The Journal

OF THE

Royal Aeronautical Society

A Monthly Illustrated Magazine devoted to all subjects connected with the Navigation of the Air.

EDITED FOR THE

COUNCIL OF THE ROYAL AËRONAUTICAL SOCIETY

BY

J. LAURENCE PRITCHARD, Fellow.

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(FOUNDED 1897 in succession to the ANNUAL REPORTS)

Edited for the Council by J. LAURENCE PRITCHARD, Fellow

No. 169 JANUARY, 1925 VOL. XXIX

NOTICES

Election of Members

The following Members were elected at a meeting of the Council held on December 9th :—

Fellow.—Major C. J. Stewart.

Associate Fellows.—Mr. E. C. Gibbons and Mr. S. O. Smith.

Student.—Mr. J. E. Bell.

Member.—Mr. R. F. R. Pierce.

Associate Member.—Major F. R. E. Davis.

Lecture Programme

The following lectures have been arranged for the second half of the present Session :—

Thursday, Jan. 22nd, 5.30 p.m.—In the Library. Major R. V. Southwell, "Some Recent Work of the Aerodynamics Department, National Physical Laboratory."

Thursday, Feb. 5th, 5.30 p.m.—In the Library. Air-Commodore C. R. Samson, "The Operation of Flying Boats in the Mediterranean."

Thursday, Feb. 12th, 5.30 p.m.—In the Library. Colonel F. Searle, "The Maintenance of Commercial Aircraft."

Thursday, Feb. 19th, at **7.0** p.m.—At the Royal Society of Arts, Adelphi. Lieut.-Colonel L. F. R. Fell, "Light Aeroplane Engine Development." Joint Meeting with the Institution of Automobile Engineers.

Thursday, March 5th, at 5.30 p.m.—In the Library. Lieut.-Colonel C. B. Heald, "Some Medical Aspects of Air Transport."

Thursday, March 19th, at 5.30 p.m.—In the Library. Captain F. Tymms, "Practical Navigation of Aircraft."

Thursday, March 26th, at 5.30 p.m.—At the Royal Society of Arts, Adelphi. Dr. Eckener, "Modern Zeppelin Airships."

Technical Discussion

An informal Technical Discussion on "The use of the Wind Channel to Aircraft Designers," will take place in the Library, at 5.30 p.m., on Thursday, January 8th. Mr. C. C. Walker will open the discussion.

Early Aeronautical Patents

The Council desire to thank the Patent Office for the gift of the following complete specifications of early patents which were missing from the Society's

collection. This gift makes the collection practically complete from 1843 down to the present date:—

1843, No. 958989, Sunderland. 1848, No. 12337, Hugh Bell. 1854, No. 224, the Earl of Aldborough. 1856, No. 2062, the Earl of Aldborough. 1857, No. 1054, the Earl of Aldborough. 1861, No. 1929, Ponton d'Amécourt.

Mr. Orville Wright

On the occasion of the twenty-first anniversary of his first flight on December 17th, 1903, the following cable was sent to Mr. Orville Wright: "The Royal Aeronautical Society sends sincere good wishes on 21st anniversary of your first flight."

Lectures

The following list of lectures of interest before non-aeronautical institutions is printed for the information of members:—

Royal Society of Arts, Three Cantor Lectures by Dr. V. E. Pullin on "Radiological Research—A History," January 19th, 26th and February 2nd.

Royal Society of Arts, Three Cantor Lectures by Dr. Walter Rosenhain on "The Inner Structure of Alloys," February 16th, 23rd and March 2nd.

Royal Society of Arts, Three Howard Lectures by Professor J. S. Brame on "Motor Fuels," April 20, 27th and May 4th.

Institution of Civil Engineers, Lecture to Students by Mr. R. K. Pierson on "The Constructional Engineering of Aircraft," January 21st.

Institute of Transport, Colonel F. Searle on "The Future of Civil Aviation," April 6th.

Library

The following books have recently been received and placed in the Library:—"The Testing of High Speed Internal Combustion Engines, A. W. Judge; "Le Vol à voile dynamique des oiseaux," L. Breguet; "Ce que tout Aviateur doit savoir," A. Lainé; "14,000 Miles Through the Air," Sir Ross Smith; "Air Power and War Rights," J. M. Spaight; "The Air Pilot: Great Britain," Air Ministry; Special Report No. 2, Radio Research Board; "The Elements of the Lanchester-Prandtl Theory of Aeroplane Lift and Drag," H. M. Martin (reprinted from "The Engineer"); "The History of Aeronautics in Great Britain," J. E. Hodgson; "Our Atlantic Attempt," H. G. Hawker and K. Mackenzie Grieve; The Proceedings of the International Conference on Aerial Navigation, Chicago, 1893; "Hints to Meteorological Observers," 7th Edition, W. Marriott; and "Captain Ball, V.C.," W. Briscoe and H. R. Stannard.

Forthcoming Arrangements

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W. LOCKWOOD MARSH, *Secretary.*

PROCEEDINGS

FOURTH MEETING, 60TH SESSION.

A meeting of the Royal Aeronautical Society was held in the Society's Library at 7, Albemarle Street, London, on Thursday, November 13th, 1924, when a paper on "Skin Friction" was read by Professor L. Bairstow, C.B.E., F.R.S., Fellow. Lieutenant-Colonel H. T. Tizard, A.F.C. (Chairman of the Society) presided.

SKIN FRICTION

BY PROF. L. BAIRSTOW, C.B.E., F.R.S., F.R.AE.S.

Skin friction is a technical term used to cover actions which occur in a viscous fluid in the immediate neighbourhood of a solid body in motion relative to the fluid. In the common usage of the expression it does not include the resistance due to eddies but only the tangential forces arising from the shearing of the viscous fluid over those parts which are held by the solid boundary. In many cases—roughly those associated with what are called streamline bodies—the layer in which the forces due to shearing are important is very thin and is appropriately called a "skin." Alternatively the expression boundary layer is used. In the circulation theory of aerofoils developed by Prandtl and his school from suggestions by Lanchester, the conception of an aerofoil is modified so as to include the "skin." The effects of the boundary layer are all-important in determining the forces on an aerofoil and the flow outside it, but the result of recent experiments at the N.P.L. gives considerable support to the hypothesis that the net effect is a circulation of the fluid as defined by well-known expressions independent of viscosity. This is true even whilst we are unable to estimate with accuracy from mathematical hypotheses the amount of the circulation.

Sometimes the whole fluid constitutes the skin; examples occur in the experiments of Hele Shaw and in the theory of the lubrication of journals; in general these cases can be solved mathematically. To some of the solutions I shall have occasion to refer more closely at a later stage in this lecture. Taking account of all methods available, analytical, graphical and mechanical, it may be said that we are satisfactorily equipped for dealing with specific motions limited in this way. Application by Michell to thrust bearings in marine engineering practice has shown how important a correct theory may be. By processes in existence up to a few years ago, thrust bearings had been developed to carry a maximum thrust per square inch of about 80 lbs. Higher pressures than this lead to failure of the lubrication and the seizing of the bearing. Applying a theory of skin friction, Michell revolutionised the design of thrust blocks and pressures of 3,000 lbs. sq. in. can now be reached. The saving in size and cost is appreciable.

This development of theory is more simple than those required in aeronautics, but from the inception of the idea to Michell's application represents a considerable interval of time. Beauchamp Tower, in 1883, discovered that the surfaces of a shaft and bearing, when lubricated, were completely separated by a thin film of oil under pressures sufficient to support the load on the shaft. Osborne Reynolds, to whom we owe much inspiration in the study of the motion of fluids, applied the equations of hydrodynamics to the problem and accounted for the

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facts. At the hands of Stanton the full consequences of the hypothesis have recently been shown, and the slide thrown on the screen is an example taken from his paper on lubrication to the Royal Society. In discussing Reynolds' results Stanton says that a preliminary investigation indicated that it was essential for the maintenance of the film that there should be a difference between the radius of the bearing and that of the journal, the two surfaces being eccentric to an extent depending on the load, speed and lubricant. The theory is deduced from the general equations of motion of an incompressible fluid in two dimensions, which are

$$\left.\begin{aligned} \frac{\partial u}{\partial t} + u\frac{\partial u}{\partial x} + v\frac{\partial u}{\partial y} &= -\frac{1}{\rho}\frac{\partial p}{\partial x} + \nu\nabla^2 u \\ \frac{\partial v}{\partial t} + u\frac{\partial v}{\partial x} + v\frac{\partial v}{\partial y} &= -\frac{1}{\rho}\frac{\partial p}{\partial y} + \nu\nabla^2 v \end{aligned}\right\} \quad . \quad . \quad . \quad (1)$$

and

$$\left.\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} = 0\right\} \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The restrictions of (1), when dealing with a "skin" or film only, have been stated in many places—notably by Stokes. Because the motion is presumed steady the terms $\partial u/\partial t$ and $\partial t/\partial t$ vanish, and because the film is thin the terms $u\,(\partial u/\partial x)$, etc., which are the product of velocities and their derivatives are unimportant compared with the forces due to shear and pressure. Equations (1) and (2) then reduce to

$$\left.\begin{aligned} 0 &= -\frac{1}{\rho}\frac{\partial p}{\partial x} + \nu\nabla^2 u \\ 0 &= -\frac{1}{\rho}\frac{\partial p}{\partial y} + \nu\nabla^2 v \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Stokes gave these equations for the very slow motion of viscous fluid when not confined to films. In three dimensions he deduced therefrom the rate of fall of minute spheres; others have applied the equations to cylinders of elliptical section, including the flat plate and circle and a strut section, and there is no serious difficulty to be expected in applying the analysis to an aerofoil. The results, although valid, are of no use in aeronautics because the appropriate value of vl/ν is microscopic as compared with that of an aeroplane wing when supporting a flying load.

In solving (3) we note the fact that in a "skin" the fluid must be flowing in lines nearly parallel to the bounding surfaces and consequently the velocity normal to the surface will be small compared with that along it. To make use of this fact it is necessary to express (3) in curvilinear co-ordinates and to consider the relative magnitude of various terms. The manipulation is straightforward and leads to the conclusion that equation (3) may be reduced to

$$0 = -\frac{1}{\rho}\frac{\partial p}{\partial s} + \nu\frac{\partial^2 u}{\partial n^2} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

$$0 = -\frac{1}{\rho}\frac{\partial p}{\partial n} \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

s now signifies distance along the film or "skin" and n distance across it. It may be noticed that the curvature of the film is not represented in the equation and in a few moments I shall develop the theorem in detail for a more general case of interest in aeronautics. From (5) we find that the pressure p is independent of the position across the film, whilst (4) shows that it varies along the film at a rate which is proportional to the coefficient of viscosity and the rate of variation of the shearing of the oil.

Since the outer part of the film is stationary and the inner is moving with the shaft the rate of shear can readily be understood to be enormous, and $\partial p/\partial s$ as deduced from (4) may be so great as to lead on integration to the pressures of several tons sq. in. recorded by Stanton. Taking a journal 4 ins. in diameter making 188 r.p.m. in a half bearing 4.004 ins. diameter loaded with 3,420 lbs. per inch run, it appears that the least thickness of the oil film is 0.00011 ins. with a thin oil for which $\mu=\rho\nu=.233$. The relative velocity of the film surfaces is roughly 200 ft./min. and at its maximum this difference occurs in about 1/10,000 of an inch. The rate of shear is evidently very great and yet the agreement between theory and experiment is so complete that one cannot doubt the correctness of the hypothesis that the motion does not contain eddies. Further consideration of lubrication would take me too far from my subject, for the equations to be solved involve consideration of the loading of the journal and its effect on the eccentricity of shaft and bearing. Those of you who are interested will find the analysis given with the experiments in Stanton's book on " Friction."

Prandtl's Boundary Layer Theory

The essential difference between the aeronautical problem and that of lubrication arises from the absence of the second solid surface represented by the bearing. A flat plate moving in its own plane through the air carries fluid with it, but there is no obvious limit to the thickness of fluid affected by the motion. We may, with some experimental justification, assume it to be small and make this a special mathematical hypothesis. We cannot, however, use equation (3) as equivalent to (1) for any normal values of vl/ν. Prandtl* therefore retains all the terms of the steady motion and considers the consequences of assuming the existence of a skin. As in Reynolds' theory, this hypothesis leads to the result that velocities across the skin are small compared with those along it and some measure of mathematical precision can be given to this physical idea.

Let AB be a portion of the surface of a body outside which a boundary layer exists. Co-ordinate axes x and y as used in defining equations (1) and (2) will, in general, not lie along and normal to this surface. Let α be the angle which AB makes with ox at o. In differentiations with respect to s it must be remembered that α is variable. The curvature of the axis of n is arbitrary and in proceeding further it will be taken as zero.

The differential operator $\partial/\partial x$ can be expressed in terms of the operators $\partial/\partial s$ and $\partial/\partial n$ to yield

$$\frac{\partial}{\partial x}=\left(\frac{\partial s}{\partial x}\right)_y\frac{\partial}{\partial s}+\left(\frac{\partial n}{\partial x}\right)_y\frac{\partial}{\partial n} \quad . \quad . \quad . \quad . \quad . \quad (6)$$

$$=\cos\alpha\frac{\partial}{\partial s}-\sin\alpha\frac{\partial}{\partial n} \quad . \quad . \quad . \quad . \quad . \quad (6a)$$

The second differential coefficient is more complicated for

$$\frac{\partial^2}{\partial x^2}=\left(\cos\alpha\frac{\partial}{\partial s}-\sin\alpha\frac{\partial}{\partial n}\right)\left(\cos\alpha\frac{\partial}{\partial s}-\sin\alpha\frac{\partial}{\partial n}\right)$$

$$=\cos\alpha\left\{-\sin\alpha\frac{\partial\alpha}{\partial s}\cdot\frac{\partial}{\partial s}+\cos\alpha\frac{\partial^2}{\partial s^2}-\cos\alpha\frac{\partial\alpha}{\partial s}\cdot\frac{\partial}{\partial n}-\sin\alpha\cos\alpha\frac{\partial^2}{\partial s\,\partial n}\right\}$$

$$-\sin\alpha\left\{\cos\alpha\frac{\partial^2}{\partial n\,\partial s}-\sin\alpha\frac{\partial^2}{\partial n^2}\right\} \quad . \quad . \quad . \quad . \quad (7)$$

Now define the velocity in the s direction as q and that in the n direction as w, the relations to u and v being

$$\left.\begin{aligned}u&=q\cos\alpha-w\sin\alpha\\ v&=q\sin\alpha+w\cos\alpha\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (8)$$

* L. Prandtl. 3rd Intern. Math. Congress, Heidelberg, 1904.

and consider the equation of continuity (2) which becomes

$$0=\frac{\partial u}{\partial x}+\frac{\partial v}{\partial y}=\frac{\partial s}{\partial q}+\frac{\partial w}{\partial n}-w\frac{\partial \alpha}{\partial s} \quad . \quad . \quad . \quad . \quad (9)$$

The quantity $\partial\alpha/\partial s$ is the curvature of the surface; apply the special hypothesis of the boundary layer to (9). Since the motion is assumed to be almost parallel to the surface AB, it follows that w is small compared with q. On the other hand, equation (9) shows that certain derivatives of w are comparable with other derivatives of q. Prandtl introduces the idea of replacing w and n by quantities $\epsilon\Omega$ and $\epsilon\eta$ where ϵ is a small constant quantity and Ω and η are of the same general magnitude as q and s respectively. Equation (9) then becomes

$$\begin{aligned} 0&=\frac{\partial q}{\partial s}+\frac{\partial \epsilon\Omega}{\partial \epsilon\eta}-\epsilon\Omega\frac{\partial \alpha}{\partial s} \\ &=\frac{\partial q}{\partial s}+\frac{\partial \Omega}{\partial \eta}-\epsilon\Omega\frac{\partial \alpha}{\partial s} \quad . \quad . \quad . \quad . \quad . \quad (10) \\ &=\frac{\partial q}{\partial s}+\frac{\partial \Omega}{\partial \eta} \quad . \quad . \quad . \quad . \quad . \quad (10a) \end{aligned}$$

In passing from (10) to (10*a*) the term involving ϵ as a factor has been neglected and the smallness of ϵ is ensured by the choice of a sufficiently high value of Reynolds' number vl/ν.

The process begun in (10) may be applied to equation (1) and leads to the expressions

$$\begin{aligned} q\frac{\partial q}{\partial s}-wq\frac{\partial \alpha}{\partial s}+w\frac{\partial q}{\partial n}&=-\frac{1}{\rho}\frac{\partial p}{\partial s} \\ +\nu\left\{\frac{\partial^2 q}{\partial n^2}+\frac{\partial^2 q}{\partial s^2}-q\left(\frac{\partial \alpha}{\partial s}\right)^2-2\frac{\partial w}{\partial s}\cdot\frac{\partial \alpha}{\partial s}-w\frac{\partial^2 \alpha}{\partial s^2}-\frac{\partial \alpha}{\partial s}\frac{\partial q}{\partial n}\right\} \quad . \quad (11) \end{aligned}$$

and

$$\begin{aligned} q^2\frac{\partial \alpha}{\partial s}+q\frac{\partial w}{\partial s}+w\frac{\partial w}{\partial n}&=-\frac{1}{\rho}\frac{\partial p}{\partial n} \\ +\nu\left\{\frac{\partial^2 w}{\partial n^2}+\frac{\partial^2 w}{\partial s^2}-w\left(\frac{\partial \alpha}{\partial s}\right)^2+2\frac{\partial q}{\partial s}\cdot\frac{\partial \alpha}{\partial s}+q\frac{\partial^2 \alpha}{\partial s^2}-\frac{\partial \alpha}{\partial s}\frac{\partial w}{\partial n}\right\} \quad . \quad (12) \end{aligned}$$

(11) and (12) are the result of changing from rectilinear to curvilinear co-ordinates. Introducing the variables Ω and η, (11) and (12) become

$$\begin{aligned} q\frac{\partial q}{\partial s}-\epsilon\Omega q\frac{\partial \alpha}{\partial z}+\Omega\frac{\partial q}{\partial \eta}&=-\frac{1}{\rho}\frac{\partial p}{\partial s} \\ +\nu\left\{\frac{1}{\epsilon^2}\frac{\partial^2 q}{\partial \eta^2}+\frac{\partial^2 q}{\partial s^2}-q\left(\frac{\partial \alpha}{\partial s}\right)^2-2\epsilon\frac{\partial \Omega}{\partial s}\cdot\frac{\partial \alpha}{\partial s}-\epsilon\Omega\frac{\partial^2 \alpha}{\partial s^2}-\frac{1}{\epsilon}\frac{\partial \alpha}{\partial s}\frac{\partial q}{\partial \eta}\right\} \quad (13) \end{aligned}$$

and

$$\begin{aligned} q^2\frac{\partial \alpha}{\partial s}+\epsilon q\frac{\partial \Omega}{\partial s}+\epsilon\Omega\frac{\partial \Omega}{\partial \eta}&=-\frac{1}{\rho}\frac{1}{\epsilon}\frac{\partial p}{\partial \eta} \\ +\nu\left\{\frac{\partial^2 \Omega}{\partial n^2}+\epsilon\frac{\partial^2 \Omega}{\partial s^2}-\epsilon\Omega\left(\frac{\partial \alpha}{\partial s}\right)^2+2\frac{\partial q}{\partial s}\cdot\frac{\partial \alpha}{\partial s}+q\frac{\partial^2 \alpha}{\partial s^2}-\frac{\partial \alpha}{\partial s}\frac{\partial \Omega}{\partial \eta}\right\} \quad . \quad (14) \end{aligned}$$

(13) and (14) are now to be simplified by rejecting all but the highest powers of ϵ and it is here necessary to consider the value of ν. The most important coefficient associated with ν in (13) is ϵ^{-2}, and if viscosity is going to be represented in the final equation ν must be small and of the order ϵ^2. We then write

$$\nu=\epsilon^2\nu_1 \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

and re-write equation (13) as

$$q\frac{\partial q}{\partial s}+\Omega\frac{\partial q}{\partial \eta}=-\frac{1}{\rho}\frac{\partial p}{\partial s}+\nu_1\frac{\partial^2 q}{\partial \eta^2} \quad . \quad . \quad . \quad . \quad (16)$$

$$0=-\frac{1}{\rho}\frac{\partial p}{\partial n} \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

and these together with the equation (10*a*), *i.e.*,

$$\frac{\partial q}{\partial s}+\frac{\partial \Omega}{\partial \eta}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

constitute the fundamental equations of the boundary layer theory. A comparison with equations (4) and (5) will show that the theory of lubrication is a special case of them in which the quadratic terms $q\,(\partial q/\partial s)$ and $\Omega\,(\partial q/\partial \eta)$ are ignored. We see again that the pressure through the boundary layer is sensibly constant and at the outside of the boundary layer where Bernouilli's equation is presumed to hold the pressure will be that measured on the surface of the solid. The velocity a little away from the surface can then be deduced from the equation

$$p+\tfrac{1}{2}\rho q^2=\text{const.} \quad . \quad . \quad . \quad . \quad . \quad (19)$$

where p is the observed surface pressure distribution. How far this is true for an aerofoil may be judged from an experiment carried out at the N.P.L.* The test was made on an aerofoil of 18 in. chord at a speed of 50 ft. in the duplex channel (7 ft. × 14 ft.), the aerofoil stretching from top to bottom. By making holes in the aerofoil along the median section the pressure distribution was observed. An independent series of measurements gave the velocity of the air at all points in the field except the boundary layer and there is no difficulty in estimating the velocity corresponding with (19) except in the immediate neighbourhood of the trailing edge.

The general truth of (17) as applied to an aerofoil is therefore well founded, especially if we make some reservation as to its validity in the wake. The experiments also show—with a similar reservation—that the boundary layer is very thin and in fact so thin as to be beyond the reach of the instruments used. Stanton has examined the boundary layer in a pipe and found very great difficulties on account of the minuteness of the apparatus required. Even the pitot tube itself failed to read velocities in the normal manner and a special calibration was required. The difficulties of direct measurement are likely to remain formidable for some time to come, but it is desirable that as much as is possible should be done if we are to understand with any degree of precision why an aerofoil lifts.

An attempt to solve equations (16) and (18), taking the pressure from the N.P.L. experiments, is being made at the Imperial College of Science and Technology and is meeting with a gratifying measure of success. The skin friction over the leading portion of the upper surface has already been calculated and it appears that the thickness of the boundary layer there varies from one to three thousandths of the chord. There are already signs of complication due to the development of the wake, which make it appear possible that some of the assumptions of the boundary layer theory will be strained to breakdown near the trailing edge. No special difficulty is anticipated in making the calculations for the under surface.

A solution of (16) and (18) for a flat plate has been obtained by Blasius,† at one time a student under Prandtl. He adopted the hypothesis that, when a plate in a fluid is moving along its plane, the value of $\partial p/\partial s$ is small enough to be negligible. He then found that the partial differential equations could be reduced

* It is hoped that the results of these experiments will shortly be published.

† H. Blasius, Thesis Göttingen, 1907 (Zs. f. Math. u. Phys., 1908).

to a single total differential equation. The treatment now given follows that of Blasius in essentials, but its detail is new and the extension to an aerofoil is rendered practicable by this new arrangement of detail.

Having used Ω, η and ν_1 to develop equations (16) and (18) it is not necessary to retain them and I have preferred to return to w, n and ν, so that the equations to be solved are

$$q\frac{\partial q}{\partial s}+w\frac{\partial q}{\partial n}=\nu\frac{\partial^2 q}{\partial n^2} \quad . \quad . \quad . \quad . \quad . \quad (20)$$

and

$$\frac{\partial q}{\partial s}+\frac{\partial w}{\partial n}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

with the condition that on the plate w and q are zero, whilst on the outside of the boundary layer $q=\bar{q}$ (a const.).

Equation (21) can be satisfied by the usual device of a stream function ψ such that

$$q=-\frac{\partial\psi}{\partial n},\quad w=\frac{\partial\psi}{\partial s} \quad . \quad . \quad . \quad . \quad . \quad (21)$$

and in terms of ψ equation (20) is

$$-\frac{\partial\psi}{\partial n}\cdot\frac{\partial^2\psi}{\partial s\,\partial n}+\frac{\partial\psi}{\partial s}\cdot\frac{\partial^2\psi}{\partial n^2}=\nu\frac{\partial^3\psi}{\partial n^3} \quad . \quad . \quad . \quad . \quad (22)$$

Blasius modified (22) by supposing that

$$\psi=\psi_1\sqrt{s},\quad \epsilon_1=\frac{n}{\sqrt{s}} \quad . \quad . \quad . \quad . \quad . \quad (23)$$

where s is measured from the leading edge of the plate. The differential relations

$$\frac{\partial}{\partial n}=\frac{1}{\sqrt{s}}\cdot\frac{\partial}{\partial\epsilon_1}\quad \frac{\partial}{\partial s}=-\frac{\epsilon_1}{2s}\frac{\partial}{\partial\epsilon_1} \quad . \quad . \quad . \quad . \quad (24)$$

are readily deduced from (23) and the differential equation becomes

$$\tfrac{1}{2}\frac{\partial^2\psi_1}{\partial\epsilon_1^{\,2}}\cdot\psi_1=\nu\frac{\partial^3\psi_1}{\partial\epsilon_1^{\,3}} \quad . \quad . \quad . \quad . \quad . \quad (25)$$

The boundary conditions are

$$\frac{\partial\psi_1}{\partial\epsilon_1}=0 \text{ when } \epsilon_1=0 \text{ and } \frac{\partial\psi_1}{\partial\epsilon_1}\to-\bar{q} \text{ as } \epsilon_1\to\text{its value at the outside of the boundary layer} \quad . \quad . \quad . \quad . \quad (26)$$

The solution of (25) can be obtained by expanding ψ_1 in powers of ϵ_1 and this course was followed by Blasius and in part is required for the procedure here given. Let

$$\psi_1=a_0\epsilon_1^{\,2}\left(1+a_1\epsilon_1^{\,3}+a_2\epsilon_1^{\,6}+a_3\epsilon_1^{\,9}+\ldots\right) \quad . \quad . \quad . \quad (29)$$

where a_0, a_1, etc., are constants. Substituting in (25) leads to the evaluation of a_1, a_2, etc., in terms of a_0; for example

$$\left.\begin{aligned} a_1&=\frac{2}{5!}\cdot\frac{a_0}{\nu}\\ a_2&=\frac{22}{8!}\left(\frac{a_0}{\nu}\right)^2\\ a_3&=\ldots\left(\frac{a_0}{\nu}\right)^3\\ &\ldots\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (30)$$

Instead of completing the series in this way, rewrite equation (25) as

$$\psi_1 = 2\nu \frac{\dfrac{\partial^3 \psi_1}{\partial \epsilon_1^{\,3}}}{\dfrac{\partial^2 \psi_1}{\partial \epsilon_1^{\,2}}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (31)$$

and integrate with respect to ϵ_1 to get

$$\int_0^{\epsilon_1} \psi_1 \partial \epsilon_1 = 2\nu \left\{ \log \frac{\partial^2 \psi_1}{\partial \epsilon_1^{\,2}} - \log \left(\frac{\partial^2 \psi_1}{\partial \epsilon_1^{\,2}} \right)_{\epsilon_1 = 0} \right\} \quad . \quad . \quad . \quad (32)$$

or

$$\frac{\partial^2 \psi_1}{\partial \epsilon_1^{\,2}} = \left(\frac{\partial^2 \psi_1}{\partial \epsilon_1^{\,2}} \right)_{\epsilon_1 = 0} e^{\frac{1}{2\nu} \int_0^{\epsilon_1} \psi_1 \partial \epsilon_1} \quad . \quad . \quad . \quad . \quad (33)$$

Now use (29) to get

$$\frac{\partial^2 \psi_1}{\partial \epsilon_1^{\,2}} = 2a_0 e^{\frac{a_0}{2\nu} \left(\frac{\epsilon_1^{\,3}}{3} + \frac{a_1 \epsilon_1^{\,6}}{6} + \frac{a_2 \epsilon_1^{\,9}}{9} + \cdots \right)} \quad . \quad . \quad (34)$$

Integrate once more with respect to ϵ_1 in order to apply the boundary condition (26).

$$\frac{\partial \psi_1}{\partial \epsilon_1} = 2a_0 \int_0^{\epsilon_1} e^{\frac{a_0}{2\nu} \left(\frac{\gamma^3}{3} + a_1 \frac{\gamma^6}{6} + a_2 \frac{\gamma^9}{9} + \cdots \right)} d\gamma \quad . \quad . \quad (35)$$

Since the exponential is necessarily positive the integral of (35) is positive and a_0 takes the sign of $\partial \psi_1 / \partial \epsilon_1$ and is negative. With the index negative the integral will tend to a limit as ϵ_1 increases, and this limit corresponds with the limit of $\partial \psi_1 / \partial \epsilon_1$, *i.e.*, $-\bar{q}$. From this condition the value of a_0 can be found as follows:—

Let

$$\beta \equiv \left(\frac{a_0}{\nu} \right)^{\frac{1}{3}} \gamma \quad . \quad . \quad . \quad . \quad . \quad (36)$$

and substitute in (35) to find

$$-q = 2a_0 \left(\frac{\nu}{a_0} \right)^{\frac{1}{3}} \int_0^{\left(\frac{a_0}{\nu} \right)^{\frac{1}{3}} \frac{n}{\sqrt{8}}} e^{\frac{\beta^3}{3!} + \frac{\beta^6}{6!} + \frac{11}{9!} \beta^9 + \cdots} d\beta \quad . \quad (38)$$

The table below gives approximate values of the integral.

$$-\int_0^e \frac{\beta^3}{3!}+\frac{\beta^6}{6!}+\ldots\ d\beta$$

β				
0	...	...	...	0
.2	...	...	...	.200
.4	...	...	...	.399
.6	...	...	...	.594
.8	...	...	...	.783
1.0	...	...	...	.960
1.2	...	...	...	1.120
1.4	...	...	...	1.260
1.6	...	...	...	1.374
1.8	...	...	...	1.464
2.0	...	...	...	1.531
2.2	...	...	...	1.578
2.4	...	...	...	1.609
2.6	...	...	...	1.627
2.8	...	...	...	1.637
3.0	...	...	...	1.642
3.2	...	...	...	1.644
3.4	...	...	...	1.645
			Lt.	1.645

. . . . (39)

These integrals were found by assuming values for β, calculating the index of the exponential, etc., and plotting on β; areas were found by taking the mean ordinate and it is not improbable that the last figures of (39) are unreliable. The final result differs from that given by Blasius by less than 1 per cent. From (38) and (39) we now have

$$-\bar{q}=-1.645\ .\ 2a_0\left(\frac{\nu}{a_0}\right)^{\frac{1}{3}}$$

or

$$a_0=-.1676\sqrt{\frac{\overline{q^3}}{\nu}}\ .\ .\ .

Using S for the area lb equation (42) becomes

$$R = 1.34\,\rho S \bar{q}^2 \sqrt{\frac{\nu}{\bar{q}b}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (43)$$

and the drag coefficient k_D is equal to

$$k_D \equiv \frac{R}{\rho S \bar{q}^2} = 1.34 \sqrt{\frac{\nu}{\bar{q}b}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (44)$$

Equation (44) shows that the drag coefficient is a function of a Reynolds' number ($\bar{q}b/\nu$) and decreases as the viscosity decreases. Had the skin friction been proportional to the square of the speed, k_D would have been constant. Equation (43) shows that, other things being constant, the resistance of the flat plate varies as $\bar{q}^{1.5}$.

So much for the theory; how does it compare with experiment? The most complete series of observations extant have recently been obtained by Burgers and Zijnen in the wind channel of the aerodynamical laboratory at Delft.* The channel is 80 cm. square and the length available for experiments 400 cms. long. The plate was of glass 167.5 cms. long, 40 cms. wide and 1.2 cms. thick. The leading edge was sharpened over a length of 15 cms. with a radius of curvature of 75 cms.

It should be noted here that the conditions are not precisely those of the calculation which assumes an unlimited dimension across the stream. Some appreciable effect of the three dimensional motion might be expected, but as yet we do not possess any other type of result. The experiments of Froude and Zahm were also made on plates of small aspect ratio.

The velocity of the air in the boundary layer was found by the use of hot wire anemometers; the diameter of the platinum medium wires was 0.005 cms. and close approach to the surface was possible before the presence of the glass had an appreciable effect on the readings of the instruments. A very large number of readings was taken in series during each of which the channel speed was kept constant. At some distance behind the leading edge the hot wire was traversed normally to the surface and a series of readings taken, the whole being repeated for a succession of positions of the normal along the median line of the plate.

The results of one series of observations are shown in Fig. 1, the velocity

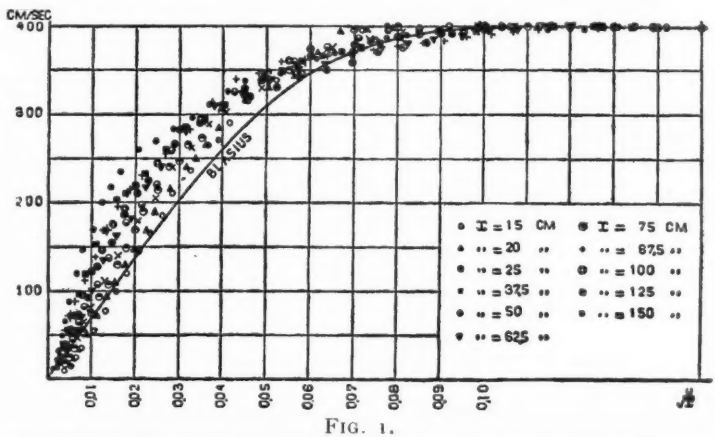

FIG. 1.

* B. A. Van Der Hegge Zijnen. Thesis Delft, 1924.

being plotted as ordinate on a base of $n/\sqrt{s}$. The curve is that calculated by Blasius, *i.e.*, by the formula in equation (38), whilst the mass of points shows the observations. The difference between observation and calculation increases as the distance from leading edge increases, but there is no doubt that the formula is a good first approximation to the observations, at least in the fore part of the plate.

The further observations of Burgers and Zijnen make it clear that for the rear part of the plate some new consideration arises which Burgers attributes to the beginning of turbulence in the boundary layer. The direct evidence is not yet published, but the records shown in the next figure will no doubt be taken as giving great support to the hypothesis.

From the observations the values of the surface friction at all points along the plate and at a series of velocities have been deduced (Fig. 2). The non-dimen-

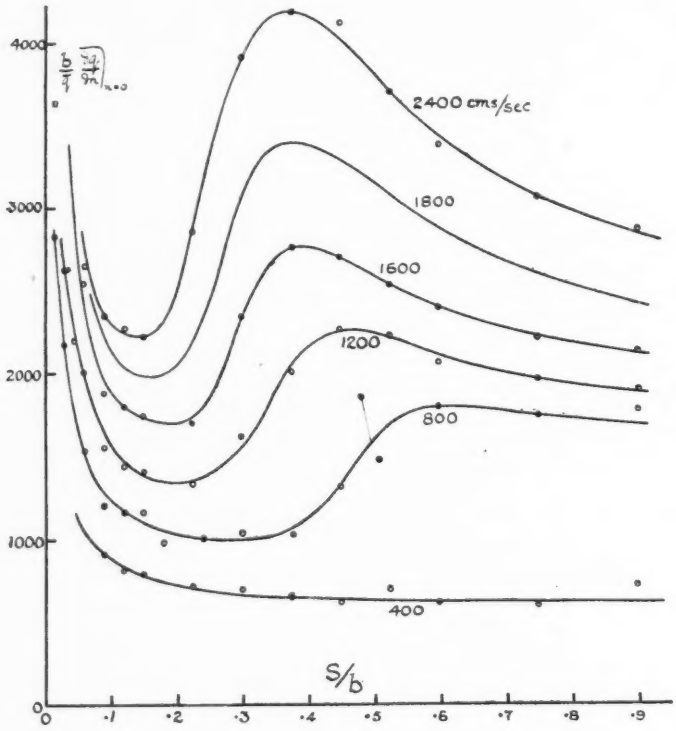

FIG. 2.

sional quantity corresponding with this surface friction is appropriately taken as

$$\frac{b}{q}\cdot\frac{\partial q}{\partial n}_{n=0} \qquad \ldots\ldots\ldots \quad (45)$$

where b is the breadth of the plate. The abscissa is s/b where s is the distance along the plate from the leading edge and b its total breadth.

The curve for a speed of 400 cm./sec. shows a regular fall, but at 800 cm./sec.

a minimum of 1,000 is reached at about $s/b = .3$, after which the curve rises sharply. At higher speeds the minimum occurs for smaller values of s/b. This marked rise in the curve is probably an indication of the beginning of turbulent motion.

A figure taken from the original by Burgers and Zijnen shows this point in relation to the Blasius formula (Fig. 3). From the observations an estimate was

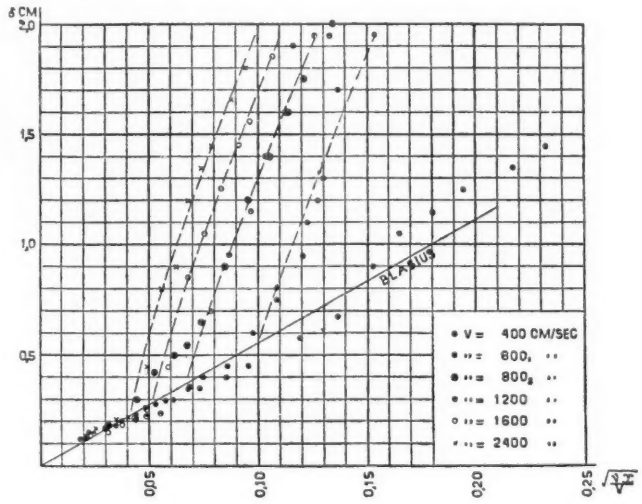

FIG. 3.

made of the thickness of the boundary layer; this quantity is rather indeterminate and possibly the use of values giving 90 per cent. of the maximum velocity would be better. The estimated thickness of the skin (δ) are plotted on a base of $\sqrt{vs}/q$ and as will be seen, the curves start with a slope of about 1 in 2, but develop a marked increase to about 3 in 1, the departure occurring at smaller and smaller values of $\sqrt{vs}/\bar{q}$ as $\bar{q}$ increases. From the figure Burgers came to the conclusion that the value of $\sqrt{v\delta}/\bar{q}$ at the bends of the curves was constant. The accuracy of the observations, however, is not sufficiently great to establish his fact beyond dispute.

The full line is drawn in accordance with the theory of Blasius and its slope is not very certain since an exponential curve, such as we used in (38), approaches its limit very gradually. The agreement of the curve and observations up to the point at which turbulence sets in is not unsatisfactory. Another figure (No. 4) showing $\partial u/\partial n$ as a function of s, indicates an appreciable divergence between calculation and experiment even before turbulence begins.

In view of these comparisons you may be tempted to regard the deductions from the boundary layer theory as a rather poor approximation to the truth and this may ultimately prove to be the case. An examination of the position suggests caution and the need for further inquiry since obvious defects exist.

Putting these defects in order, we have:—

(1) The experiments described on the glass plate do not represent two-dimensional motion and are therefore not strictly comparable with the calculation. The effect of the rounded front edge may not be

unimportant, and in any case errors due to lateral spreading of the air may be expected to increase with distance from the leading edge.

(2) The motion may be turbulent and the difficulties of dealing with a fluctuating motion may be formidable.

(3) Loss of pressure along the plate is ignored in the theoretical calculations; although small the proportional effect on the skin friction may not be unimportant.

(4) The Prandtl equations are mathematical approximations.

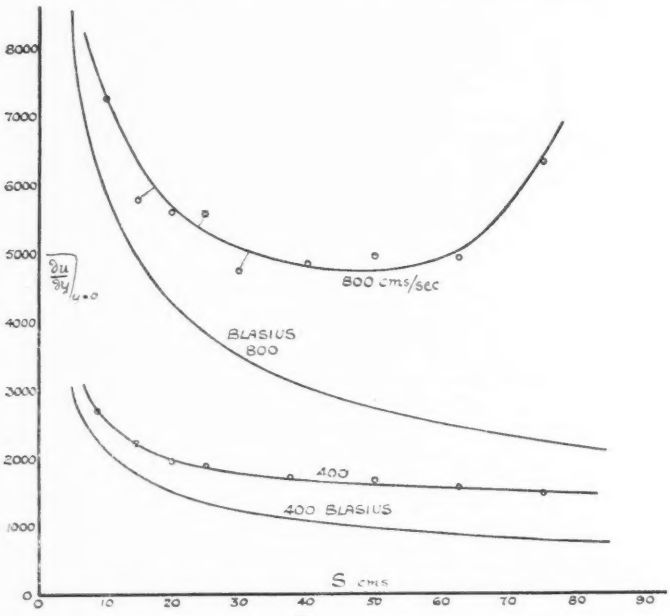

FIG. 4.

Of the four defects, Nos. 1, 3 and 4 can probably be removed at an early date. In the application to an aerofoil special care was taken at the N.P.L. to ensure that the experiments corresponded very closely with two-dimensional motion. The pressure distribution was observed and its effects are so important that they must be taken into account at once; it is clear that the texture of the skin of an aerofoil is greatly different from that on a flat plate and that no deduction can safely be made from one to the other. The fourth difficulty is one which can probably be dealt with when once a solution of the equation to the order of approximation given by Prandtl has been obtained.

Whilst this line of inquiry is one which I think should be pursued, it is not one which can be regarded as satisfactory as at present derived. More is possible from an entirely different standpoint, but the applications to aeronautics will perhaps not be reached so quickly. Fortunately the study of aerodynamics as a theory of fluid motion has now received international recognition and progress is assured as a result of the primary interest of a number of individuals in this fascinating but very difficult subject.

Discussion

Major Low, in opening the discussion, said that Professor Bairstow had done much less than justice to Lanchester in crediting him only with "suggestions" from which Prandtl and his collaborators had developed the circulation theory of lift.

Actually, in his "Aerodynamics" (1907), Lanchester had published, for all the world to read, a remarkably complete exposition of the theory, as was fully recognised by German writers though full recognition was still unaccountably withheld here.

It would have taken Professor Bairstow a few hours to verify roughly the existence of the circulation appropriate to the lift for any wing, and perhaps a few weeks to put the theory on an unassailable experimental basis. Yet here they were 17 years after the publication of Lanchester's book and 15 years since the formation of the Advisory Committee for Aeronautics, and it was difficult to gather whether Professor Bairstow had yet accepted the theory or not.

There was therefore an historic irony in the fact that Lanchester in his "Aerodynamics" had also given a satisfactory theory of skin friction identical in principle with Prandtl's theory of the boundary layer, and had thus anticipated the paper before them by 17 years.

On re-reading the discussion on Lanchester's paper, "The Flying Machine" (Vol. IX., Proc. Inst. Aut. Eng., 1915), one could not fail to be struck how consistently Lanchester had been in full possession of the modern point of view both of the circulation theory and of "skin friction," and how completely his critics had failed to rise to his level of insight into the physics of the problem.

He noticed that, in answer to a remark by Baumhauer at last year's London International Air Congress (p. 295), Professor Bairstow had stated (p. 296) his interest in hearing that at the Innsbruck International Conference (1922), continental leaders of thought had realised the considerable importance of introducing viscosity into the theory of aerofoils.

Again at the end of the paper before them he expressed satisfaction that "aerodynamics as a theory of fluid motion had *now* received international recognition and that progress was assured," etc.

Did he realise that Prandtl had read his original paper on the boundary layer in a viscous fluid before the International Mathematical Congress at Heidelberg in 1904? The state of affairs is, *not* that continental mathematicians and physicists are waking up to the importance of aspects of the subject recently pointed out in this country, as one might easily infer from these remarks, but that our aerodynamicians were just beginning to waken up to the importance of methods and results in which we were from ten to twenty years behind Germany, thanks largely to our neglect of Lanchester.

He himself had been misled into ignoring Lanchester. He had spent a great deal of time on the theory of surfaces of discontinuity (which is still the staple diet in our text books) and had uttered constant complaints about the unreality of hydrodynamics in relation to flight, of which at one time Professor Bairstow was a recipient. In 1921 a Swiss scientist had advised him to read Prandtl, as one who had devised a method of building up approximate differential equations by studying the physical phenomena, and thus had made great progress in bringing reality into the theory. He had followed this advice and had quickly come into knowledge of the great achievements of the German school and of Lanchester's high place in their esteem.

It was not necessary to reproduce Lanchester's work there. Rayleigh, by using the analogy between distribution of velocity along the normal at equal space intervals on a flat plate of finite breadth, and at equal time intervals on an infinite plane, wrote down the result (after integrating across breadth b).

Tangential resistance per unit area $= 2\pi^{-\frac{1}{2}} (\nu/Vb)^{\frac{1}{2}} \rho V^2$. This was identical with Lanchester's result, save that $2\pi^{-\frac{1}{2}}$ replaced an undetermined numerical coefficient c.

Carrying the argument further, they must assume an accumulation of retarded fluid near the finite plate, to satisfy the equation of continuity.

They might replace the retarded fluid approximately by an "equivalent" layer (of the order of 1 mm. in depth) of equal total momentum and vorticity but with uniform velocity gradient along the normal.

Assuming that the mean velocity was reduced to $\frac{1}{2}V$, this doubled the depth of the layer, in turn reduced the tangential forces to half, and thus finally reduced the doubled layer to the same depth as the original uncorrected layer.

The correction was clearly in excess and the resulting value of $c = \frac{1}{2} \times 2\pi^{-\frac{1}{2}} = .56$ was a lower limit.

Assuming that the equivalent depth was increased by $2^{\frac{1}{2}}$, and the shear reduced by $2^{-\frac{1}{2}}$, they got a balance of effects, and $2^{\frac{1}{2}}\pi^{-\frac{1}{2}} = .80$ appeared as an accurate solution, subject to uniform retardation.

But the assumption of equal retardation throughout the boundary layer did not allow for the fact that retardation and consequent reduction of shear was in fact greatest at the surface. Hence, the last figure is an upper limit.

Finally

$$2\pi^{-\frac{1}{2}} > 2^{\frac{1}{2}}\pi^{-\frac{1}{2}} > c > \pi^{-\frac{1}{2}}$$
$$\text{or } .80 > c > .56.$$

The geometrical mean is .67. The exact value (corresponding to Prandtl's differential equations) is given by E. Polhausen as .664. Thus a slight extension of Lanchester's simple physical reasoning places c between reasonably close limits of which the geometrical mean gives more than sufficient accuracy (max. possible error ± 20 per cent.) in view of the complications pointed out by Lanchester as arising from the bluffness of the nose and the appearance of turbulence.

Lanchester had claimed, justly, that his theory of "skin friction" set up a lower limit of wing resistance which the designer could never quite attain.

The numerical coefficient c appeared in the kinetic theory of gases, and in the theory of heat transfer between solid and fluid, as well as in the theories of skin friction propounded by Lanchester (1907) and Prandtl (1904). The exact value was determined by E. Polhausen in the problem of heat transfer.

K. Polhausen, in a paper (1921) referred to by Baumhauer (*loc. cit. supra*), had given an exhaustive comparison of various approximate solutions of Prandtl's differential equations, and the problem might be regarded as worked out in so far as the laminar motion was stable.

Professor Bairstow's paper made the subject more accessible to English readers, and it was an admirable training for his pupils, our future aerodynamicians it might be, that they should be brought into direct contact with Prandtl's methods and results.

If one, whose conscience was not altogether clear, might say so, it would be better to avoid detailed exposition of mathematical analysis on the blackboard as far as possible. While the lecturer was writing and speaking he could neither see what he wrote nor hear what he said, and though thoroughly familiar with the subject could not have made connected notes. That sort of thing, like Shakespeare, was more suited for private study than for public declamation.

But when the lecturer went on to physical interpretations and their comparison with experiment, his discourse might have been acceptably three or four times as long. His summing up was admirable, and his conclusion that turbulence consequent on instability was the only outstanding difficulty, was in agreement with German views and he thought with the facts. It should be noted that

some progress had been made in Germany in applying to turbulent motion the statistical method suggested by Kelvin and applied by Osborne Reynolds.

There were two points on which they had been in controversy in the past. The first was raised by the assertion that the work in this paper was less fundamental than work on the solution of the general equations of viscous flow. The second was the validity of the general equations of motion of the boundary.

With reference to the former, the general equations of motion of a viscous fluid in their most compact form, consisted of a vector equation with seven terms representing forces arising in seven distinctive ways and the (scalar) equation of continuity with two terms.

He had gone to the trouble of expanding the vector equation in the most general orthogonal curvilinear co-ordinates and had obtained 128 terms each of which might require further expansion in three or six or more terms; the equation of continuity was not quite so prolific. To talk of solving such a system brought no concrete image of procedure to his mind's eye. In contrast, Prandtl's final differential equation for the boundary layer contained four terms!

The other point was the validity of these equations at the boundary. Professor Bairstow had once said *ex cathedra* that the writer's view, that their validity at the boundary was open to question, was "just not true." The writer had since found his views sustained by Kelvin, and by Riabouchinsky, and he would be glad to meet Professor Bairstow in a technical discussion on the subject. He had better add at once that possibly they had been at cross purposes rather than in direct opposition.

The relevance of the writer's remarks on criteria of instability and, if he might say so, the fundamental importance of that subject had apparently not been realised, and he proposed to hand his notes on the subject to the Secretary as a possible basis for a technical discussion.

He would conclude by saying that the vigour of his criticisms arose from his measuring Professor Bairstow's work by the extraordinarily high standard set in Germany. Lest he misled those who were unfamiliar with his belief in introducing a certain liveliness into discussion he would add that it gave him great personal pleasure to recognise Professor Bairstow as filling worthily our senior chair of aeronautics, and as having given them one of the most stimulating and significant papers he had listened to in that Society.

Dr. LEVY said that in reading Professor Bairstow's paper he had found one difficulty, especially now that he had seen the slides. In equation (9) it was legitimate to neglect the term $w\,(\partial u/\partial s)$ when it was small in comparison with the other terms, and this Professor Bairstow had done; but this term was proportional to the curvature, and in the neighbourhood of the nose of the plate this would necessarily, in any practical case, become exceedingly large. It would appear, therefore, that the approximation might not be expected to apply in the region of the nose but be valid further along the plate; how far along, he did not know. A comparison between theory and experiment for, say, the shear on the surface should be expected to bring this out. An examination of the last slide shown, however, indicates the direct opposite, viz., that agreement is good in the forward region and rather indifferent for the rear part. He could not understand how this arose.

Dr. STANTON said that he had listened to Professor Bairstow with great pleasure and profit, and in his own case there was a feeling of gratitude to him for having cleared up a number of points that had caused trouble at the N.P.L. Dr. Stanton explained how the experiments on the theory were conducted at the N.P.L. and quoted examples. One of the difficulties was to find what was the actual resistance very close to the leading edge which, if it could be determined, would constitute the best check on the accuracy of the Blasius formula.

They had tried experiments at the N.P.L. on thin rings that ought to give results somewhat nearer to the values calculated from the formula than in the case of plates only 4ins. wide. The rings were 1.8 long. He had worked out that afternoon the values and the agreement was very close.

Another interesting point was the variation of the resistance with the speed. The index of V in the Blasius formula should be 1.5. That was a matter which they had considered quite recently. Miss Marshall had been experimenting with the rings and had plotted the results in the ordinary logarithmic manner. They were rather surprised to find that the value of the index was little over 1.6 and they had been unable to account for that until Professor Bairstow's lecture was before them, and then they seemed to see the reason for it. The agreement was satisfactory. It was a little odd that rings similar to these, when tested on the wind channel balance, had not shown this low value of the index.

Professor Bairstow had mentioned the experiments which had been made at the N.P.L. with three-sided pitot tubes. They had thought that the method was going to be of considerable value for general exploration work, but it had not turned out to be so, for the reason that close to the leading edge of a plate the boundary layer was very much thinner than further down stream, and so the method could not be applied there.

Professor Bairstow had mentioned something which he had not quite been able to follow about the effect of turbulence on this boundary condition. There is a very old experiment that has often been made. If one put a streak of colour in water flowing through a glass pipe, it would be seen that at a certain distance from the inlet the streak broke up into eddies. That was the beginning of turbulence. The interesting point was that by no increase of the speed could the turbulence be made to occur at the mouth. Reynolds had pointed that out, but had not offered an explanation. Professor Bairstow had apparently suggested that there might be a change in the skin friction at the point where turbulence began, but no satisfactory evidence of such a change had yet been found at the N.P.L.

He was sure the Society was very grateful to Professor Bairstow for the trouble he had taken in bringing the subject before them.

Dr. N. A. V. PIERCY said that many of those who might have been engaged on work relating to skin friction would be anxious to apply the methods of reasoning that Professor Bairstow had set before the meeting in so interesting a fashion. Meanwhile, one experimental result had appeared in the course of channel work at East London College which had a direct bearing on the paper, and he would also like to call attention to a difficulty which seemed to exist in the application of the subject to aerofoils, on which Professor Bairstow might be able to throw a valuable light at this early stage.

Dealing with the experimental result first, he said that the pitot boundary round the front portion of the upper surface of an aerofoil eighteen inches in chord had been found. There was a difficulty in assigning an exact thickness to the boundary layer in this region from the readings owing to the considerable diameter (about 0.5 millimetre) of the pitot tube employed. Assuming, however, the pressure in the tube to be appropriate to the fluid level with its centre, experiment gave the thickness of the boundary layer as about 1.4 thousandths of the chord at the nose and 3 thousandths at about 0.2 chord downstream. As these figures for thickness tallied very closely with those calculated in the paper, it would be interesting to know whether the range downstream was also roughly the same.

Coming to the special difficulty he had found with the subject, this was involved in apparently divorcing some effects of the external system of flow from the motion within the boundary layer. Professor Bairstow had not dealt exhaus-

tively on the present occasion with the aerofoil side of the problem, and he therefore hoped further question was not premature, but the lecturer might, nevertheless, possibly be in a position to amplify the information already given. Considering for clearness the case of an element of the aerofoil surface situated at its extreme crest, and taking the aerofoil to be moving through air initially at rest, the fluid at the outer edge of the boundary layer did not of course remain stationary, but was hurled backwards with a velocity comparable with the forward velocity of the element. After a first reading of the paper, one would like to know whether, in view of the consequent large and quick variation of $\bar{q}$ along the chord, w had to be paid special attention to. In a loose way, one might look upon the traction at the element of surface as being partly contributed to by the external system of flow. This might, he thought, increase the thickness of the boundary layer rather quickly. Although the experiments already quoted did not permit of a clear issue on the point, there was some indication that the increase in thickness became rather rapid well within the given range downstream. In any case it was known that losses due to the external motion accumulated and resulted, long before the trailing edge was reached, in a considerable thickness of the boundary layer—not, of course, to be associated with tractions alone.

If it were found that the present methods of analysis did indeed break down at an early stage along the chord, as in view of the foregoing he thought there was reason to anticipate, it appeared to him that methods founded on the flow of momentum or energy might prove useful in pushing the investigation further. He thought considerable advantage might accrue to a concerted effort to thresh out from an entirely theoretical standpoint the proper manner in which such methods might be applied.

Major R. V. Southwell said that while Professor Bairstow had been concerned that evening with calculations relating to steady motion, he had mentioned the suggestion of Burgers and van Zijnen, that the reason why agreement between their measurements and Blasius' theory broke down at some little distance behind the leading edge was the occurrence of instability due to very high rates of shear which existed in the boundary layer. It seemed to him that that was a very reasonable thing to expect, and extremely likely to be true in view of the work which Karman had conducted in Aachen, starting from some work of Blasius. Blasius had examined all the published data in relation to the drop of pressure down long circular tubes, and had devised a satisfactory dimensional formula for that rate of drop; Karman used this formula as a foundation for a theory of the motion in the boundary layer when that motion was not steady but eddying, and by semi-empirical but satisfactory methods he had been able to evolve a theoretical expression for the resistance on flat plates of any length (that is to say, he had predicted how the resistance coefficient should vary with length). It was quite extraordinary how accurate were the results obtained by his method, in circumstances where assumptions similar to those of Lanchester, in the work described by Major Low that evening, would give completely different estimates.

He (Major Southwell) was of opinion that where the boundary layer theory described by Professor Bairstow broke down turbulence was the cause, and that the origin of turbulence was instability—instability of a kind quite different from that discussed in Professor G. I. Taylor's work, and by Major Low that evening, and having its origin in the intense rates of shear in the boundary layer. He had been trying for some six years now to devise theoretical methods for investigating instability of this kind, but had not yet succeeded, although he believed he had made some progress.

He wished, like Dr. Stanton, to express a personal sense of regret that he had been unaware of the work which Professor Bairstow had described for so

long a time after its publication. It was necessary to face the fact that Prandtl's boundary layer theory had been published in 1904, and Blasius' calculations for the thin plate in 1907, so that both had been available for two years when the Advisory Committee for Aeronautics began its work in 1909! For his own ignorance, of course, no one was to blame but himself; but he would like to ask whether we were satisfied with the share which this country had taken in recent developments of fundamental hydrodynamic theory. The work of Professors Bairstow and Taylor was, of course, a contribution of immense value, and the progress which they had made in their researches might be expected to continue; but he believed that there were others who had the mental equipment required for these very difficult but (as it seemed to him) quite essential investigations, and that what was really lacking in aeronautical circles to-day was a sufficiently real and widespread interest in the fundamental problems of hydrodynamics to obtain for these investigations the encouragement which they required. If we really wanted them to progress we must be prepared to pay some price for them; it was of no use merely to place them on a paper programme along with a large number of experimental investigations, and trust to a pious hope that somehow, in the intervals of other work, the new ideas would be forthcoming.

Colonel TIZARD, in proposing a vote of thanks to Professor Bairstow, said that at this late hour he would say no more than that he agreed with Major Southwell in attaching practical importance to scientific work on fluid motion. There was always a danger of postponing such work in favour of investigations of more immediate practical interest. Professor Bairstow was an old friend of the Society; and the length and contentious nature of the discussion on his paper was a measure of his power to make an abstruse subject interesting.

Dr. B. G. v. d. HEGGE ZIJNEN.

Delft, December 15, 1924,
v. Leeuwenhoeksingel 16.

To Prof. L. Bairstow,
Imperial College of Science and Techn., South Kensington, London.

Dear Sir,—As I learned from an abstract from your paper read before the Royal Aeronautical Society, on Thursday, Nov. 13th, relating to the problems of skin friction in aeronautics, you kindly referred in your paper to the experiments on the velocity distribution in the boundary layer along a plane surface, carried out at Delft and which were the subject of my thesis. Unfortunately my thesis (communication 6 from the Aerodynamical Laboratory at Delft), the preliminary measurements on this subject (communication 5 from the Aerodynamical Laboratory) and the paper read by Prof. Burgers on these experiments before the International Conference on Applied Mechanics at Delft in April of this year have led to a misunderstanding concerning the principle of the experimental arrangement and the evaluation of the experimental results.

Although the apparent lack of accuracy of my experiments was underlined in the discussion following your paper by many speakers and the words spoken have reached many since, I would like to bring the following corrections to your attention, in the hope that they may contribute to remove the unfortunate light thrown by a misunderstanding on these experiments. I had already the pleasure of sending you the thesis mentioned and the communication 5 (preliminary measurements).

You stated in your paper that the leading edge of our glass plate was rounded and that this would have an appreciable effect on the experiments, as the turbulent state of motion would in this case occur already in the first part of the boundary layer. However, in communication 5 the remark was made that the leading edge was sharpened (p. 11) at both sides; it had the following form: radius of curv. = 75 cm. The effect of the leading edge was studied separately and the form chosen proved to be most suitable to avoid the turbulence in the

first part of the boundary layer and could not have influenced, as was remarked in the discussion, the experimental results very much, which may be clearly shown by a closer examination of my thesis.

The lateral spreading of the air in the channel was determined (not neglected, as was stated) separately, an account of this was given in communication 5. The velocity distribution of the air over the cross section of the channel was measured to this extent by means of a pitot tube 15 cm. behind the glass plate, also two velocity curves in the boundary layer were determined at 150 cm. from the leading edge of the plate (where this effect, if any, would be detected most clearly) at the middle of the height of the glass plate and 15 cm. above. In the first case no trace of lateral spreading could be detected and in the second the mutual differences of both velocity curves were at the most one per cent., so we may say that lateral spreading is out of question. This can easily be explained, as Mr. Southwell pointed out at the International Conference on Applied Mechanics at Delft, by the fact that the air in the channel can find no way, due to the suspension of the plate in the channel, with less resistance then over the plate. A simple calculation has confirmed this.

The pressure drop in the channel along the plate was measured and it was found that it had an influence on the experimental results and therefore it was taken into account in the evaluation of the results.

As I must fear that this misunderstanding will not be in favour of the experiments carried out at Delft, and your criticisms were of fundamental character, you would highly oblige me by accepting these corrections.

Yours truly,

B. G. v. d. HEGGE ZIJNEN.

In reply, Professor BAIRSTOW said: Major Low again raises the question of the apportioning of merit between individuals concerned in the development of aeronautics and gives to Lanchester a share greater than that which appeals to me as fair. The discrimination is distasteful and I propose to leave the personal aspect as presented in the paper and discussion without any attempt at reconciliation. For my own neglect and lack of understanding I make apology, for I realise that behind Major Low's vigorous criticism lies much to which it is desirable to draw attention. Whether it be lack of clearness in the presentation of new ideas, lack of appreciation of them when presented, or defects in the circulation of published material, it is unfortunately true that progress is impeded by imperfect contact between workers in a given subject. I had heard of the boundary layer theory of Prandtl before 1914, but it was not until Easter of this year that I became aware of the solitary solution by Blasius, and this as a result of a visit to Delft for an International Congress on Applied Mathematics. The difficulty is not peculiarly British, for continental workers are not fully conversant with developments here. It is perhaps pertinent to remind Major Low that for ten years international scientific intercourse has been in abeyance and that the process of dissemination of knowledge—never rapid in one's own country—has been deliberately interfered with. Many of us would welcome developments which will enable workers to come into effective contact with each other.

As to dates, I have added one or two references to the paper, all of which, together with others, will be found in the thesis by Dr. Zijnen to which frequent reference has been made. These dates are in agreement with those quoted by Major Low.

Professor Levy draws attention to the violation of certain approximations at the leading edge of the flat plate. This difficulty requires arguments which—probably from my lack of knowledge of German—I have not found in the accounts of the solution by Blasius. I am the more ready to accept the approximation as sufficient from work by Mr. Cave, Miss Lang and myself on the application of Oscar's equation to the skin friction on a flat plate. In that case the differential

equation solved takes in part of the inertia terms, but is confined to small values of vl/ν and therefore not comparable with the Blasius solution, an essential element of which is that Reynolds' number is to be very large. It does, however, show the same feature due to infinite curvature of the leading edge. Not only is the rate of shear infinite, but the pressure is infinite and compressibility is therefore certain to be involved. The more complete mathematical analyses there possible showed that the region of high pressure and stress is extremely local and its integral over the front part of the plate not so great as to invalidate the application to the rear portions. The general conclusion is that the extreme nose is not satisfactorily dealt with, but that the front part of the plate is not therefore necessarily in serious error. The more marked failure at high speeds for points still further back remains without adequate theoretical explanation.

Dr. Stanton's experiments on thin rings appear to approach more nearly to the conditions postulated in the theory than any others known to me, and it is interesting and gratifying to find that the agreement with prediction is also better than any yet found. I look forward with interest to the publication of results which Dr. Stanton now promises. The relation between experiment with a streak of colour and the Delft experiments is beyond any explanation I can offer. Presumably the turbulence which ultimately shows itself throughout the tube is propagated radially from the sides, the shear which starts at the end of the pipe taking time to travel to the centre. I cannot see reason why the results found by Zijnen on a flat plate should fail to show in the experiments at the N.P.L. on tubes.

The author has to thank Dr. Piercey for some curves amplifying his remarks on the thickness of the boundary layer as referring to an aerofoil. Although this matter is rather beyond the scope of the paper, it is clear that some of the inferences by Dr. Piercey are of importance. From the point of least pressure on the upper surface of an aerofoil it is clear that the normal velocity w becomes more and more important. Up to this point the pressure gradient assists the matter whilst, of course, skin friction retards it; behind the point both pressure gradient and skin friction retard the flow and the whole energy lost must be taken from kinetic energy in the air. It may be that herein lies the possibility of improving aerofoils, particularly with respect to maximum lift coefficient, for the effect of skin friction could be reversed by moving the fabric backwards on the top rear half of the wing at a speed rather greater than that of the wind.

Major Southwell referred to the statistical work of Karman for use in unsteady motion. The thesis by van Zijnen shows in addition to the results described in my paper a comparison of theory and experiment in the turbulent layer. The discussion of these points was deliberately left out of the scope of my paper to avoid undue length, but the subject is one which I should like to see dealt with at one of the lectures to the Royal Aeronautical Society. I am pleased also to note the hearty support for better contact with other workers and the plea for a wider recognition of the importance of a study of the fundamental problems of hydrodynamics. This point was further emphasised by the Chairman.

A communication from Dr. Zijnen shows that, unfortunately, he has not obtained from the accounts reaching him the impression which it was intended to convey. I hope that he will accept my assurance that I do not question the accuracy of his experiments and was very grateful for the valuable information his thesis contains. His letter is reproduced with the discussion and his points are replied to below.

The expression " rounded front edge " to which Dr. Zijnen takes exception occurs in the summary towards the end of the paper. In describing the plate earlier it was stated that " the leading edge was sharpened over a length of 15 cms., with a radius of curvature of 75 cms.," so that it may be hoped that the impression Dr. Zijnen desires to avoid has not been produced. The second point

of the lateral spreading of the air is one which I still think open to some doubt as to magnitude which is not wholly removed by the experiments to which attention is specially drawn. In comparing theory and experiment it appears to me to be legitimate to make this point generally and also for Dr. Zijnen to contend that its effects cannot be large. The last point is clearly one of misinterpretation; in my summary I state that one of the defects is that "Loss of pressure along the plate is ignored in the calculations." From the context it is clear that the "theoretical" calculations by Blasius were meant and not the reductions of observations made at Delft. It may perhaps be less ambiguous in an amended form where the word "theoretical" has been used to qualify "calculations."

PROCEEDINGS

FIFTH MEETING, 60TH SESSION.

A Meeting of the Royal Aeronautical Society was held in the Library at 7, Albemarle Street, London, W.1, on Thursday, November 27th, 1924, Lieutenant-Colonel H. T. Tizard (Chairman of the Society) presiding. A paper was read by Dr. G. C. Simpson, C.B.E., F.R.S., on "Thunderstorms and Aviation."

The CHAIRMAN, in introducing Dr. Simpson, said that they had the honour of welcoming there that night the Director of the Meteorological Office, who would give them a lecture which must be of very great importance to anyone interested in aviation.

Dr. SIMPSON then read his paper.

THUNDERSTORMS AND AVIATION

A thunderstorm has always been an object of interest to the human race; it is supposed to have been the source from which primitive man first obtained fire; it has always been associated with magic:

"When shall we three meet again,
In thunder, lightning or in rain?"

croaked the witches in Macbeth; its awe has appealed to the poet, its beauty to the artist, its wonder to the scientist, and now in these latter days its danger has appealed to the aviator.

It frequently happens that when we know all about a danger we cease to fear it. Although I propose to tell you to-night as much as I can about thunderstorms in the time available, I cannot promise you that the result of my lecture will be to make you fear a thunderstorm less; on the contrary, I shall be very surprised if your respect for this meteorological phenomenon is not considerably enhanced before I have finished.

When most of us think of danger in association with a thunderstorm, we think only of the danger from lightning flashes, but this is in my opinion a small danger compared with the less spectacular danger of contending air currents.

Everyone must have been impressed with the evidence of great forces at work in a thunderstorm. The enormous damage done by lightning and the destruction caused by the heavy rainfall are obvious to the most casual observer, but probably few have any idea of the magnitude of the energy involved. Recently Mr. C. T. R. Wilson* has been able to make actual measurments which give a definite numerical value to the energy associated with a lightning flash, and he finds that the energy of an average lightning flash is of the order of 10^{17} ergs. Reduced to more familiar units, this is 3,700 horse-power hours. But to get a real idea of what this means we can best compare it with the electrical power generated in all the electrical undertakings in London. On this basis we find that a single lightning flash would keep every electrical undertaking in the London and Home Counties Electricity District supplied with power for eight minutes.†

* C. T. R. Wilson, Phil. Trans. A., Vol. 221, p. 106, 1920.

† During the year ending 31st March, 1924, 1,768,070,741 units of electricity were generated in this district; *vide* "Generation of Electricity in Great Britain," published by His Majesty's Stationery Office, 1924.

The electrical energy released in a thunderstorm lasting for two hours with one flash every second would supply the whole of the British Isles with electrical energy for ten days, and this would be a very moderate storm.

Equally stupendous quantities of energy are connected with the rainfall. In a storm in the neighbourhood of London on June 16th, 1917, 6,000,000 tons of water fell on an area of fifty square miles in $2\frac{1}{4}$ hours. All this water fell at least 3000 feet, so that the work done in the $2\frac{1}{4}$ hours was at the rate of 150,000 h.p. and would have supplied London with electrical energy for three days if it could have been passed through efficient turbines.

Where does all this energy come from? It would take me too far into rather difficult questions of thermodynamics to give a complete answer to this question; but it will probably be sufficient for our purpose if I say that it comes from two main sources, these being warm air in the neighbourhood of cold air and water vapour.

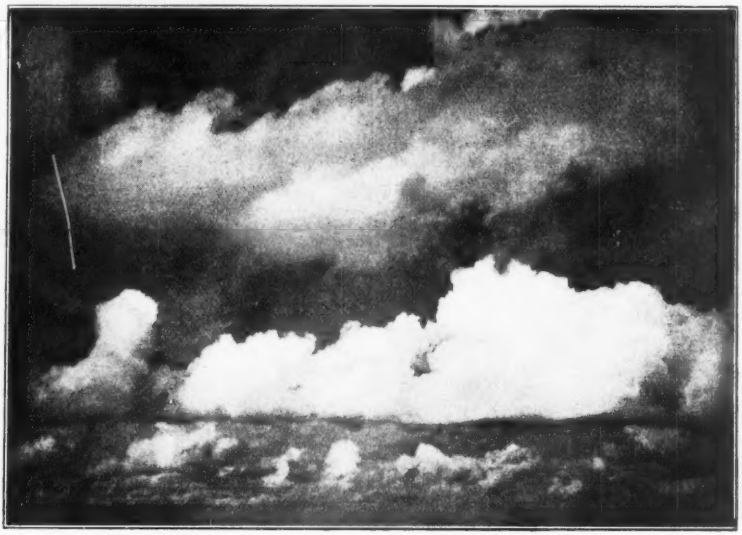

FIG. 1.

Warm air may be in the neighbourhood of cold air in two ways; in the first the air near the ground may be too warm in relation to the air above it; this is the case on a warm summer afternoon, and gives rise to what have been called heat thunderstorms; in the second there may be warm and cold currents side by side. In either case a great deal of energy must be released as the air gets into a more stable condition.

The energy associated with water vapour is due to the fact that when water vapour condenses into liquid water or solid ice a considerable amount of heat—up to that time latent—is evolved and warms up the air, in some cases actually raising its temperature.

We will first consider the heat thunderstorms which occur on warm days in the summer.

You are all perfectly familiar with the sequence of events which leads up to such a thunderstorm. The early morning is generally sunny, with very few

clouds, but during the course of the morning small cumulus clouds appear in all parts of the sky, as shown on this photograph* (Fig. 1).

Each one of the clouds marks the top of a column of ascending air, and all aviators are familiar with the bumps experienced as one flies in and out of these columns of air on such a day.

As the day proceeds and the surface gets warmer, the columns get closer together and the cloud heads coalesce, until finally the ascending currents in one region predominate and this region becomes the chief region of ascent, into which the surrounding currents become attracted and we get one relatively large ascending current with a great cloud mass at the top. This is the typical thunder cloud, of which I show you a picture (Fig. 2).

Looking at this photograph, it is easy to picture the processes taking place. The mass of cloud consists of relatively warm air, warm because it has risen

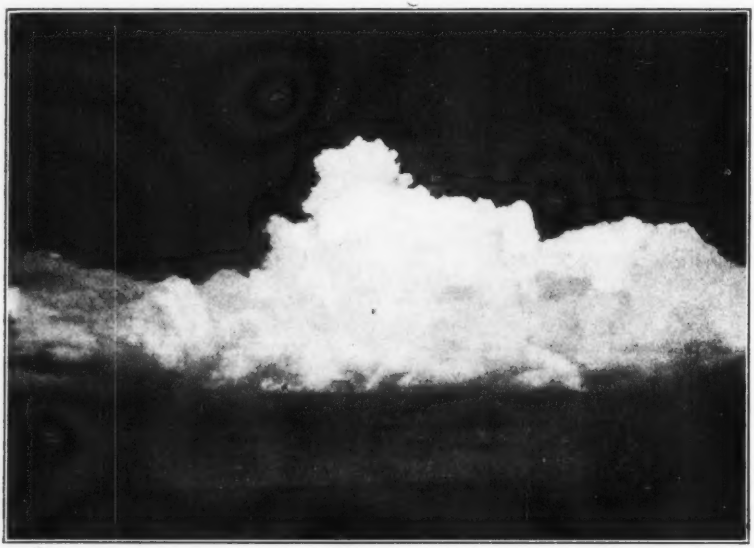

FIG. 2.

from the warm lower layers and because the condensation of the water vapour into cloud particles has released enormous amounts of latent heat. This warm air is pushing its way into the relatively cold and clear air which surrounds it. A cloud of this size obviously has not been produced simply by the air immediately below it rising upwards. Suitable warm air has had to be drawn in from all sides to keep up the supply. In fact, such a cloud is much like a chimney which draws in air at the bottom. At first the air may come in from all sides, but when the thunderstorm is fully developed the air stream comes in from only one direction and we get a complicated system of air motion, to which I will refer later.

We also notice the beautifully rounded domes at the top of the cloud—commonly called thunder heads. These show that the cloud is rising unimpeded through colder air, with which it is not mixing. But our atmosphere is not

* I am indebted to Mr. G. A. Clarke, of the Meteorological Observatory, Aberdeen, for the photographs and sketches reproduced as Figs. 1, 2, 3, 5, 8, 9, and 10.

uniform; it really consists of a number of layers having different temperature characteristics. In some of these layers the temperature is relatively high, and when the top of the cloud reaches such a layer it cannot rise any higher, for now it is not warmer than its surroundings. The layer forms a ceiling and the cloud has to spread out under it. At the same time the sharp boundary of the cloud breaks down and the edges, instead of being clear cut, become ragged and spread out like teasled wool. A cloud in this state is now shown on the screen (Fig. 3).

There you see very clearly the way the cloud has stopped rising when it has met the relatively warm layer or ceiling and spread out under it. The whole form of the cloud now resembles an anvil, and this is so typical a form that meteorologists usually refer to these clouds as "anvil clouds."

We will now consider more closely the air motions which accompany thunderstorms, for they are very characteristic and of great importance to aviators. I

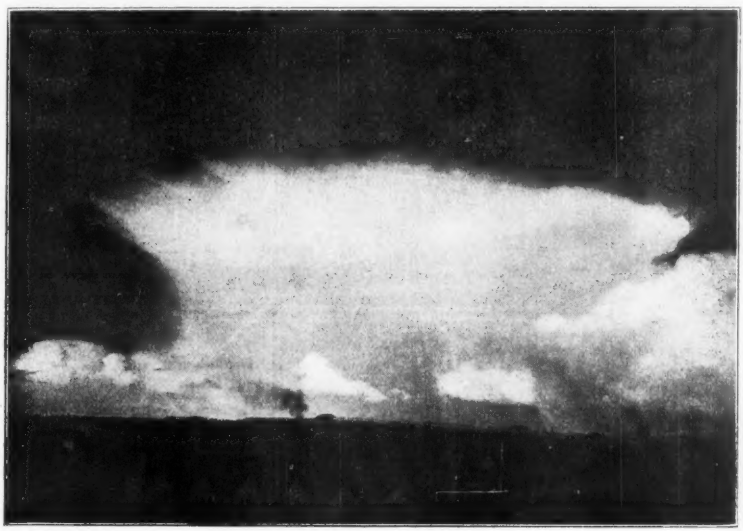

FIG. 3.

should preface my remarks on this part of the subject with the statement that we can only deal with a highly idealised thunderstorm. In reality the air currents in a thunderstorm are very complicated, especially when several storms exist near to one another. Also we have very few actual observations of the currents except near to the ground and practically none within the cloud itself. You will therefore find that other meteorologists have put forward slightly different schemes, but the diagram which I now show you (Fig. 4) has been prepared to fit in with the largest number of facts recorded from the neighbourhood of thunderstorms, and I think well represents the main features.

In this diagram the air motion is shown by the arrows, the length of which is drawn roughly proportional to the wind velocity. It must be remembered, however, that the air motion shown is relative to the thunderstorms, if the storm is moving as a whole in a general air current the velocity of this current must be added to the air motion shown.

The dotted lines represent the paths followed by the falling precipitation, and again nothing more than the general characteristics have been shown.

It will be noticed that the air currents may be divided into two main classes —those which feed into the storm and those which pass out underneath it.

For some distance in front of the storm the air blows towards it. This air is really being drawn in by the ascending currents, and is very similar to the "draught" which feeds a chimney. This air comes from near the ground, and is therefore warm and damp. It rises as it nears the storm, and enters through the lower front part of the cloud, becoming an ascending current which feeds into all parts of the cloud.

When one looks at a large thunder cloud from the outside it appears to be formed of the same kind of cloud particles from the base to the top. This, however, is not the case, and the physical state of the cloud is of great importance especially to aviators who may have to fly into such a cloud or to a balloonist who may get drawn into one.

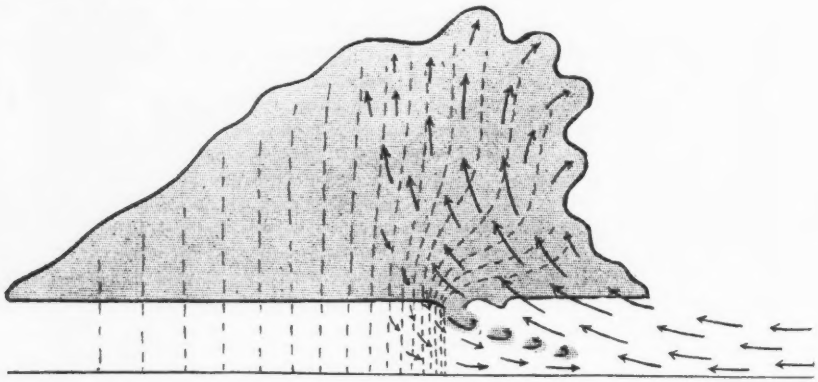

FIG. 4.

Let us consider a cloud which forms on a warm summer afternoon with a temperature of 70°F. and a relative humidity of 75 per cent. As this air rises, cloud will form at a height of 1,500 feet when the temperature is 62°F. This is the height of the base of the cloud. The air and the cloud particles continue to rise, and when they have reached 12,000 feet the temperature will have fallen to the freezing point. It must not, however, be thought that the cloud particles will freeze. As a matter of fact, it is extremely difficult to freeze cloud particles and the temperature has to be lowered to the neighbourhood of the zero on the Fahrenheit scale—that is, to over 32 degrees of frost—before all the cloud particles turn into ice. This temperature is reached in our present example at about 25,000 feet; that is extremely high up in the atmosphere, and many thunderstorms fail to reach this height. We have then to think of a thunder cloud as consisting of three portions. In the lower portion the cloud particles are composed of water above the freezing point. In the middle portion the majority of the cloud particles are still in the liquid state, but they are supercooled and are ready to freeze if they get the opportunity. This opportunity would be given if an aeroplane flew into this portion of the cloud. Every cloud particle which touches any part of the aeroplane will immediately turn into ice, and the wings, struts and wires of the aeroplane will soon be covered with ice in the form of thick hoar frost. Most aviators are familiar with this state of a cloud. As one ascends in this portion of the cloud, more and more cloud

particles are met with which have turned into ice and are floating as minute ice crystals mixed up with the supercooled water drops. The colder the air becomes, the more the crystals predominate, until finally we reach the third stage, in which all the cloud particles are ice crystals.

In all three stages of the cloud, water vapour is being condensed and water is separating out; what, then, will be the state of the precipitation in each layer? By far the greatest precipitation takes place in the lower layers of the cloud, and here the precipitation is evidently in the form of rain. But the ascending currents are generally much too strong to let the drops fall, and the water is literally blown up with the ascending air. One would expect that these drops would finally freeze and fall as hail, but hail is not frozen rain—at least a hailstone is not a frozen rain drop.

FIG. 5.

I had hoped to be able to explain to you how hailstones are formed, as it is a very fascinating subject, but I find that time will not allow. I must content myself by saying that only very small hailstones could be formed unless the stones have been supported by ascending air currents and prevented from falling for quite a considerable time; in fact, most hailstones have been actually blown upwards by ascending currents. This gives us practically the only method we have of estimating the rate of ascent of the air in a thunderstorm. We can easily calculate the upward velocity necessary to support a hailstone of a given size, and as we know that every hailstone has been supported, we get at once a measure of the upward velocity. Hailstones of half an inch in diameter are very common, and these need an upward current of 33 feet per second (22 miles per hour) to support them. Hailstones as big as pigeon eggs, and even as big as hen eggs, are frequently reported. These indicate ascending currents of enormous velocities, regular vertical hurricanes, in which no aircraft could survive for a fraction of a minute.

These violent ascending currents carry vast quantities of water vapour with them, most of which is condensed out and falls sooner or later as rain or hail.

The descent of this precipitation, which is naturally cold owing to its place of origin, tends to drag the air down with it, and in those parts of the cloud away from the main ascending currents it actually does cause a strong downward motion of the air. This downward current is deflected by the ground and moves outwards in the front of the storm. Everyone is familiar with the heavy gust of cold wind blowing outward from under the cloud and raising clouds of dust and leaves as a thunderstorm approaches.

This cold air, then, has been dragged down with the precipitation and is cold, not because it has descended, but because it has been mixed with the cold rain and hail. As this cold current meets the warm indraft which is feeding the storm, there is much eddy motion, through which the warm and cold air mix and cause cloud formation at a considerably lower level than that of the base of the cloud. This effect is shown in the accompanying photograph, in which the wisps of cloud indicate where the two currents are meeting (Fig. 5).

The air currents in this part of the storm are often real whirls around horizontal axes as indicated in the diagram, and are extremely dangerous to aircraft. I shall return to this again.

So far I have dealt only with what are generally called heat thunderstorms, in which the energy is due to the surface layers becoming overheated. I have now to describe the thunderstorms caused by warm and cold currents being brought together. This frequently occurs with the air currents associated with barometric depressions. Around the west side of such depressions there is a cold current bringing air from polar regions which comes up sharp against the south-west current in the south of the depression. Here are conditions which cannot help but cause trouble. The warm and cold currents cannot possibly exist side by side; the cold current cuts under the warm current which rises with much turbulence and violent ascending currents. The final effect is very similar to the case where the instability has been brought about by surface heating, but there are one or two great differences which are important from the point of view of the aviator.

In the first place there are much more violent squalls associated with thunderstorms of this nature, and in the second place these storms extend along lines of very great extent which move forward right across the country with considerable velocity; in fact, they have received the name of "line squalls."

Fig. 6 shows the meteorological conditions associated with a line squall of this nature. The lines on the diagram are isobars and the figures show temperatures in Fahrenheit degrees. The arrows fly with the wind and the feathers indicate the force. We see here very clearly the two currents—a westerly current over Ireland, Scotland and the north-west of England and a south-westerly current over the south and south-east of England. The temperature difference is not so clear owing to the lower layers of the atmosphere being affected by the temperature of the ground, but from the figures shown on the diagram it can be seen that the former current is the colder and is no doubt of polar origin. Along the line where these currents meet there is a large discontinuity of pressure, and it is this discontinuity which gives rise to the heavy squalls.

The squall line was first detected over the Hebrides at 1 a.m. on the morning of February 8th running in a south-west—north-east direction. The line maintained this general direction but moved forward at right angles to its length right across the British Isles and northern France as shown in Fig. 7, in which the lines indicate the position of the line squall at successive hours. It took fourteen hours to travel from the Hebrides to London—that is at a rate of about thirty-six miles an hour. The intensity varied greatly at different parts of the country, but when the intensity was greatest, thunderstorm occurred as shown by the symbols entered on the chart. As the line passed over a place there were

heavy squalls and a sudden change of wind direction from S.W. to W.N.W., with a rapid drop in temperature.

One of the most characteristic features of such a line squall is a peculiar roll cloud which forms along the front. A photograph of a pastel sketch of such a cloud at Aberdeen is shown in Fig. 8. This cloud forms when the cold

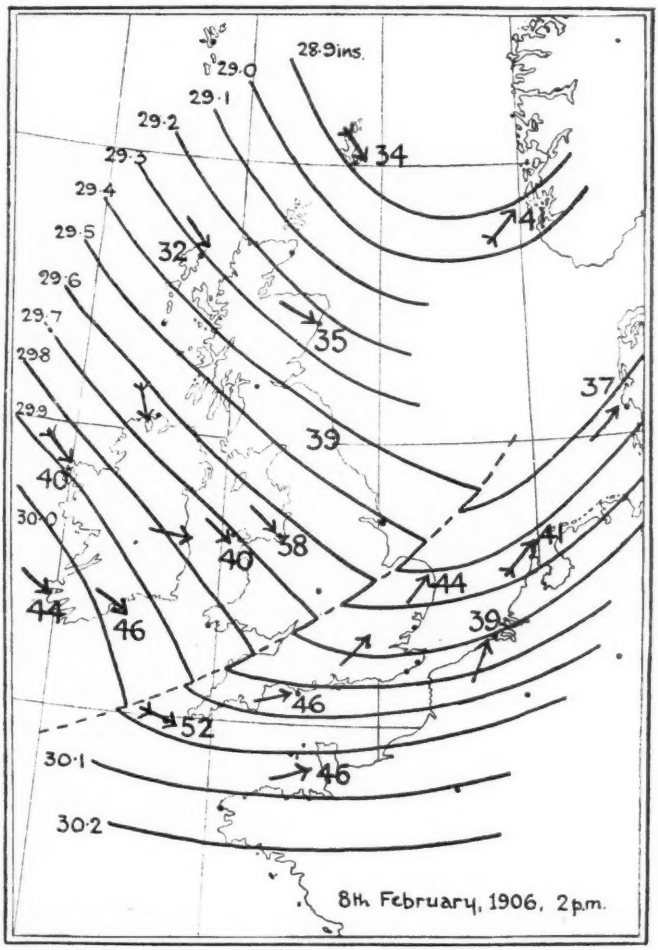

FIG. 6.

and warm currents meet, and is really a violent vortex around a horizontal axis. As the cloud was moving in a south-easterly direction it passed over the sea near Aberdeen. The air over the sea was relatively warm and damp, and as this air was drawn upwards into the cloud the mixture of warm and cold air produced the same kind of wisps of clouds as we have already discussed when considering the front of a heat thunderstorm. Fig. 9 shows the cloud in this

stage. Later the cloud lost its sharp outline and the formation of cloud extended almost down to the sea, when three water spouts appeared between the cloud and the sea surface, showing the great instability of the air. This stage is shown in Fig. 10.

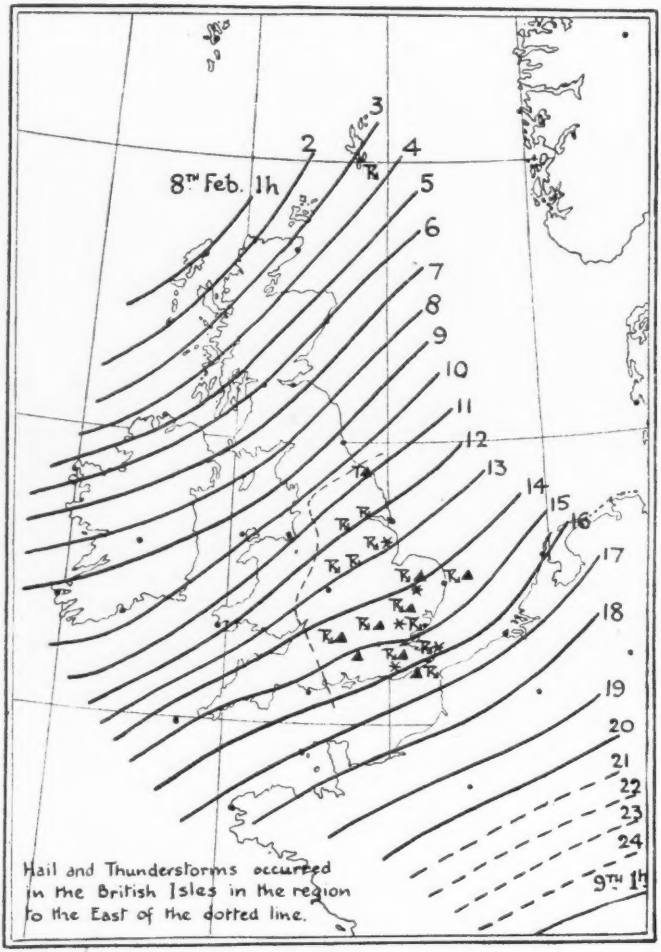

FIG. 7.

If for a moment we neglect the effect of lightning, the following are the chief characteristics of thunderstorms which are of interest to aviators.

(*a*) The indraft into the storm which is of particular danger to free balloons.

(*b*) The violent whirls in the front of the storms which are dangerous to all aircraft, but especially to airships.

(c) The ascending currents within the cloud mass from which probably no aircraft has yet escaped after being once caught.*

(d) The heavy precipitation—both rain and hail—with which is associated strong downward currents. This precipitation damages the fabric of aeroplanes and balloons alike and reduces visibility, while the downdraft produces loss of height which may well be fatal.

I propose to give very shortly a few accounts of actual experiences of aircraft in thunderstorms to illustrate each of these points. I may, however, remark that the material is not extensive. Aviators do not fly into thunderstorms if they can possibly avoid them, and of those who do get drawn in, few survive to tell the tale.

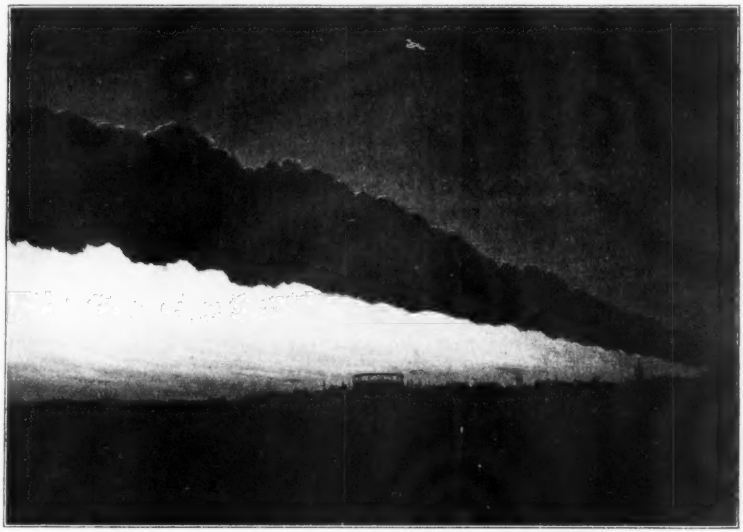

FIG. 8.

While the memory of the appalling catastrophe associated with the Gordon Bennett race of last year is fresh in the minds of everyone here, I need say little about the danger of free balloons being drawn into the centre of a thunderstorm. I shall later give the account of the only survivor from the three destroyed balloons; here I will simply say that each balloon got into the current feeding the storm, and before they knew their danger they were involved in the ascending currents in the storm cloud from which they could not escape.

As an example of the danger of the whirls in the front of a line squall, the following account of the loss of the German Zeppelin L.1 on September 9th, 1913, may be given.

The airship left Hamburg in fine weather to take part in manœuvres with the German Navy near Heligoland. By the time it reached Cuxhaven light rain was falling, which became heavier as it passed on to Heligoland. Just before the catastrophe the airship was in a thick fog and the rain was falling as if there had been a cloudburst, so that the water streamed into the cabins.

* This refers to the violent currents where the electricity is generated and the hail stones are supported.

The weight of the water pressed down the ship, which was caught in the air currents blowing into the squall.

According to observations made on a German warship, in seven minutes the height of L.1 varied from 450 feet, 240 feet, 900 feet and 600 feet. Immediately before the catastrophe the variation became still greater, and within a few seconds the airship rose from 300 feet to over 4,000 feet and then fell rapidly, struck the water with a velocity of 60 feet a second, broke into several parts and rapidly sank.

I cannot find an account of an aviator who has actually flown in an aeroplane in the heart of a thunderstorm, but there are many accounts of aeroplanes being violently thrown about when flying near or within a thundercloud.

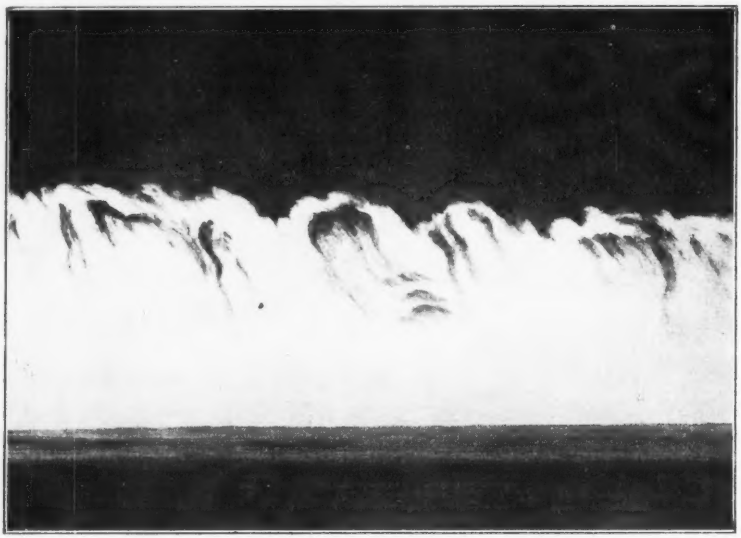

FIG. 9.

The following account of a balloon in a thunderstorm appears in Wegener's "Thermodynamik der Atmosphare" and can be accepted without reservation. It agrees with what we should expect and is related by a well-known German aeronaut.

"We ascended from the exercise ground of the Luftschifferbataillons on a dull sultry summer afternoon. Soon the wind died away completely and the air became oppressively close. After about an hour, during which the cloud layer slowly descended, we found ourselves over a marshy clearing in the forest and we decided once more to throw out ballast in order to reach drier ground. After pouring out about half a bag of sand we slowly rose from the drag rope and began to prepare our frugal evening meal in peace and quiet. Suddenly the air became remarkably cold. A glance at the barometer showed that we had rapidly risen over 6,000 feet. We were soon in a dense, formless, grey mist, out of which came irregular gusts of wind shortly followed by heavy rain and hail. Simultaneously began a moaning and hissing accompanied by heavy rolls of thunder on all sides. We were caught up in a whirl which set the basket swaying first slowly and then violently from side to side like a pendulum. Suddenly we fell 3,000 feet or so only

to be immediately raised again as rapidly as we had fallen. This violent up and down motion was repeated over and over again for half an hour during which period great hailstones poured over us from all sides, so that a layer of water and hailstones nearly a foot deep covered the bottom of the basket. [There seems no doubt that during this period they were caught in the whirl which forms between the warm rising current and the cold descending current.] It is quite impossible to describe the violent swinging and swaying of the balloon in the stormy whirling air currents. The swaying was so extreme that occasionally we were at the same level as the balloon, the cordage which bound the basket to the balloon being at one moment so tightly stretched that it cracked under the tension and the next moment so loose that the ropes hung slack about us.

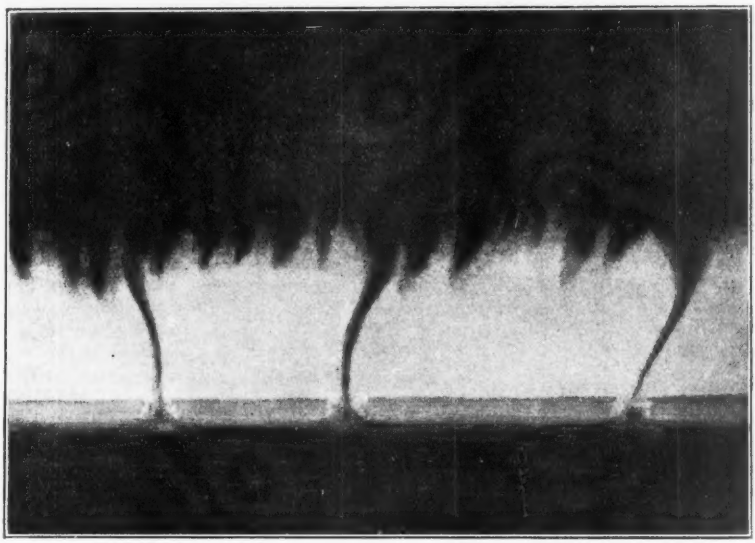

FIG. 10.

"Finally the loss of gas caused by the turbulent motion was so great that the ascending current, in which we had been held up like a glass ball on a fountain, could no longer support us and we began to fall at a terrific rate. For three minutes we fell from a height of 7,000 feet at a rate of about 40 feet a second and it was only because the balloon fell into a wood that we were saved from a fatal accident."

So far my account of thunderstorms has been rather like *Hamlet* without the Prince of Denmark, for I have said practically nothing about the electrical phenomena to which these storms owe their name. As a matter of fact, however, the thunder and lightning which accompany thunderstorms are a mere by-product of the forces which produce the storm. If there were no thunder and no lightning the number of storms would not be reduced; we should simply have to find a new name for them. Still, from the aviator's point of view the electrical state of the thunderstorm is a very important one, although probably overrated, and must now be considered.

The origin of the electricity in thunderstorms is a very old problem and is still the subject of considerable controversy, no explanation being accepted by all meteorologists. This is not the place to enlarge on this controversy so I will con-

tent myself with giving an outline of the theory which for the time being is most generally accepted.

I have already explained that the centre of a thundercloud is the seat of very violent ascending air currents, some idea of the magnitude of which can be obtained from the size of the hailstones which they have supported. As the air in these currents ascends large quantities of water are precipitated and rain-drops are formed which commence to fall relatively to the ascending air. A rain-drop however cannot fall at an unlimited rate through air.

A small drop falls only very slowly, soon reaching a steady rate of fall which depends on its size. A water-drop the size of a cloud particle falls at a rate of less than one foot a second, while a drop having a diameter of about a quarter of an inch falls at a rate of twenty-five feet per second. If therefore the air is ascending at a greater rate than this, drops a quarter of an inch in diameter and all smaller drops will be carried upwards and will not fall at all. Now it is a remarkable and important fact that no drop having a larger diameter than a quarter of an inch can exist for more than a fraction of a minute. It is unstable and breaks up into a number of smaller drops. From this you will see at once that no water can fall through an ascending current of more than 25 feet per second. All the water then which is precipitated in the more rapidly ascending currents—and the more rapid the current the greater the quantity of water precipitated—is carried upwards and tends to accumulate in the cloud where the velocity of the ascending currents falls to 25 feet a second on account of lateral spreading out. Here the water is going through a rapid process of forming large drops only to be broken up again into smaller drops. Now laboratory experiments have shown that every time a drop of water breaks up electricity is separated, the water obtaining a positive charge and the air a negative charge. This distribution of the resulting charge is important, for on account of it the positive electricity remains low in the cloud in the water while the negative electricity, which is in the air, is rapidly carried away into the upper parts of the cloud. In this way a very effective method of producing and separating electricity is active in the cloud. The separation of electricity goes on until the accumulation becomes so great that enormous electrical forces are set up, in part between the positive and negative electricity in different portions of the cloud and in part between the charged cloud and the ground. It is these forces which give rise to the discharge which we see as lightning and hear as thunder.

The physical processes involved in a lightning flash are very complicated and somewhat difficult to explain without using technical language. If, however, you will allow me a certain freedom in description I will try to give you a short account of the method in which a lightning flash is formed. The description will I hope be accurate so far as it goes, but will not be complete in detail. I have explained how the electricity is separated in the cloud and that this separation produces strong electrical forces. These forces are likely to be strongest in the lower part of the cloud where there is the greatest accumulation of positively charged water. At some point in this strong field the force will rise to the well-known value of 30,000 volts per centimetre. This is the strength of field which air cannot support, the molecules of air are torn apart with the emission of light, and this portion of the cloud becomes a conductor of electricity. Now introducing a conductor into the field in this way concentrates the force. In consequence the conducting region is extended in the direction of the force, and what is equivalent to a rent is torn through the air for great distances, often extending right down to the ground. Such rents, which we call lightning flashes, go in both directions, up into the cloud where they cannot be seen except as a general illumination of the cloud and down to the ground where they are visible and can be photographed. There is one point which I wish to emphasise and that is that it is only necessary for the strong field of 30,000 volts per centimetre to occur in a small region of the electrical field. It is often stated that this field strength must exist along the whole length of the flash before a discharge can take place. This is wrong, for if the discharge starts

anywhere, because of the high local field, the discharge itself, by making the air a conductor, concentrates the field in its neighbourhood and the rent is continued far into regions where the original field was relatively weak.

As soon as a conducting channel, or rent, has been formed in the manner described, electricity flows along it until the cloud is discharged. Here I am not telling the whole story as only a part of the cloud can be discharged owing to the cloud being a non-conductor of electricity, but for practical purposes we can say that the cloud is discharged. In any case no more electricity flows, the discharge stops and the channel ceases to glow. But the electricity goes on accumulating in the cloud and soon there is again enough to start another flash, but the old channel is still available, for the air takes time to return to its normal state, and the flow starts again in the old channel. This process is sometimes repeated several times.

It is possible to show these effects by photographing lightning flashes with stationary and moving camera. The photograph which I now show on the screen (Fig. 11) was taken with a stationary camera by Prof. B. Walter of Hamburg.

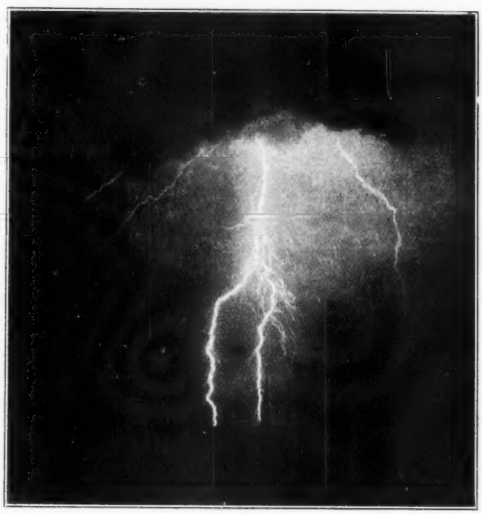

FIG. 11.

You see what appears to be two lightning flashes which may have occurred simultaneously or in rapid succession; it is impossible from this photograph to say which. The flash however was photographed at the same time with a camera revolving about a vertical axis and the second photograph was the result (Fig. 12) There you see six clear flashes. On the right of the picture is the first flash, which is obviously the same as the vertical flash on the stationary picture. Then at irregular intervals five other parallel flashes are shown. If you examine the photograph closely you will see that the upper part of the second flash down to the arrow is exactly similar to the same part of the first flash. It appears from this that the second flash followed the first channel down to the arrow and then for some unknown reason left the old channel and found another channel for itself. The third and subsequent flashes followed the channel made by the second flash from top to bottom. The total interval between the first and last flash was just over half a second. I also wish to draw your attention to the ribbon-like appearance

between the fourth and fifth flashes. This is due to the fact that the flow of electricity did not entirely stop after the fourth discharge had taken place, but continued with diminished intensity for an appreciable time.

It does not need a moving camera to show this effect. Fig. 13* was taken with a stationary camera, but it shows in a remarkable way the ribbon-like effect. The reason for this is that the conducting channel moved as a whole with the wind and the discharge was so long drawn out that the channel moved sufficiently far during the discharge to show as a broad band in the photograph.

Before going on to discuss the effect of the electricity of thunderstorms on aircraft from the theoretical point of view, I should like to interpolate here an account of the loss of the Spanish balloon " Polar," one of the three balloons which were struck by lightning in the Gordon-Bennett race in 1923. This account has been taken from the German paper " Luftfahrt " for April 1924.

FIG. 12.

The sole survivor of the three crews, Captain Felix Gomez Guillaumon, pilot of the ill-fated Spanish balloon " Polar," has given a brief account of this flight and of his own escape. According to his story, the " Polar " went up on September 23rd, 1923, about 5 p.m. from Solbosch near Brussels. It was raining heavily, and the balloon was completely soaked, and as the pilot counted on an early landing the drag rope was already thrown out. The bad landing conditions and the gusty weather necessitated throwing out a good deal of ballast, but the pilot overdid it and the balloon rose very rapidly into dense cloud. Gomez Guillaumon was standing in the middle of the car and his passenger, Captain Pennaranda, was at the side of the basket leaning against the suspensions, when at an altitude of about 1,200 metres the cloud became less dense and there was a sudden detonation like a pistol shot. The pilot noticed no light and no sparks, nor did he feel any shock, but only saw that his companion crumpled up and fell on the floor of the car, while the side of the car was sprinkled with blood. Looking upwards the pilot saw that flames were rushing out of the inlet fitting, which was inclined sideways. Simultaneously, he heard the typical crackling of flames, rather like a smithy fire, without however feeling any effect of heat from the fire 10 metres below in the car. The balloon continued to rise and in the few seconds remaining to the pilot in

* I am indebted to Mr. B. L. Webb for this photograph.

which to act he decided to cut the lines securing the inlet fitting, but could not find his knife. The lines, however, had already been burnt through at the top by the inlet fitting ring, and Guillaumon decided to rip the balloon before the net and envelope became a prey to the flames. He was successful in ripping the balloon halfway. The crackling immediately ceased, the gas escaped and the car dropped towards the ground. After a few seconds, however, the speed of fall diminished and thence onwards until the balloon struck the ground, it remained constant at a little more than 10 metres/sec. It took rather less than two minutes to drop the 1,200 metres, and during this time the pilot was able to throw out four more bags of ballast each weighing about 15 kg. Twelve bags remained in the car as 19 out of the 35 with which the balloon started had already been used at the critical moment in the flight. With a ground wind of 40 to 50 km. the car struck the ground, overturned and the ballast bags unluckily fell on the pilot when he was pitched out, so that he lay with his right thigh broken. His companion had been killed by the lightning and he owed his own escape to his prompt action and to the fortunate circumstance that in the descent the envelope in the net formed a sort of parachute, even if only a very imperfect one.

FIG. 13.

We must now consider what effect we should expect the electricity of thunderstorms to have on aircraft. This has been the subject of much discussion, a great deal of which, I am afraid, has been badly informed. As a matter of fact, it is very difficult to say how aircraft will be affected when they come under the influence of the electrical forces associated with thunderstorms. The whole problem is extremely complicated and cannot safely be handled along theoretical lines. All that we can say is that given such and such conditions of the electrical field with an aircraft of a certain form and composed of certain materials such and such things may happen. He would be a brave man who would say exactly what would happen.

In the few minutes remaining to me I will just outline a few of the general principles which must be taken into account when considering this question. In the first place we must remember that the electrical forces are by no means always vertical, very strong horizontal fields may be encountered, and in the second place it is not only the field strength, but also the rapid changes which are dangerous.

In the case of an aeroplane I doubt whether there is much danger except that of happening by chance to be in the path of a discharge. To a slight extent the metal of the plane will deform the field, making it stronger in the neighbourhood of the plane than it would otherwise be. This might deflect the path of a flash towards the plane, but it is hard to estimate how great the effect would be. In any case I think we can safely say that the effect would not be very great unless the plane were very near—say within a hundred yards of the path of the flash—and that position would be dangerous in any case. Captain Cave has expressed the opinion that the actual danger from lightning to an aeroplane flying through a thunderstorm may be no more than that incurred by a pedestrian walking across an open common during a storm. This probably somewhat under-estimates the risk, but in general terms it is correct.

The case is different with balloons and airships because of the danger of setting fire to the gas. It has often been stated that there is no danger in balloons, because they are almost entirely composed of non-conducting material, and therefore do not deform and strengthen the field. But as a matter of fact a wet balloon is far from being a non-conductor of electricity, and if the balloon is carrying a long wet drag rope the danger may be very great.

It is the problem of the airship which is of the most practical importance at the moment.

There are three very real dangers to an airship—

(*a*) A direct lightning stroke.

(*b*) Ignition of the gas in the balloon owing to an internal spark.

(*c*) Ignition of escaping gas through an external spark.

(*a*) *Danger from a direct lightning stroke.*—There can be no doubt that the large mass of metal—or rather the large extent of the metal framework—very seriously deforms the field and may quite well raise the local electrical force up to that strength which will start a lightning flash. In any case the field is so much deformed and strengthened over such a large area that the chance of a lightning flash being attracted to the airship is very largely increased.

There is no cure for this; it is inherent in the shape and material of an airship; the only question is, can the lightning flashes be safely conducted through the ship without igniting the contained gas? There can be little doubt that the framework of the ship is massive enough to conduct the currents involved without any serious heating, except probably at the points of entry and exit, where the current is concentrated and the current density correspondingly great. If, therefore, we can arrange for the flash to enter and leave the ship in such a way that the local heating is not dangerous, the problem would be solved. The natural solution would appear to be to provide the ship with lightning conductors made of stout metal rods projecting away from the hull of the ship which would take the flash and conduct it safely to the main framework. Unfortunately, however, such rods concentrate the field so strongly that they might actually initiate flashes; and further, a rod only protects a limited area, and to protect the whole surface of the ship in this way would necessitate an impossible number of rods.

I do not think that the solution lies in this direction. In my opinion it is best to remove as far as possible all protuberances and leave the hull, especially the upper part, as smooth and regular as possible. The shape of the hull then determines where the flashes are likely to strike, and it is easy to see from general principles that in this case the pointed nose and the fins at the stern are most likely to be struck. Now these parts are the furthest away from the gasbags and also they are just the parts where the metal masses are, or may easily be made, sufficiently heavy to take safely the heavy currents of the lightning flash. I understand that Zeppelins have on several occasions been struck in

this way without serious consequences, and as a matter of fact no Zeppelin is known definitely to have been destroyed by lightning. While, therefore, one does not wish to minimise the danger of a direct stroke of lightning to an airship, there is no need to imagine that an airship will necessarily be destroyed if it is struck by lightning.

(*b*) *Ignition of the gas inside the balloon owing to an internal spark.*—Everyone is familiar with the fact that electrical charges are induced on a conducting body when it is in an electrical field. On account of this there are very large charges induced on an airship when it is in the strong electrical field of a thunderstorm. The positive charge is concentrated on one part of the ship and the corresponding negative charge on another part. If now the field is suddenly destroyed, as for example by a distant lightning flash, the force holding these induced charges apart ceases to exist and the charges rush together in the form of violent and large electrical currents. These currents set up induced currents in the various elements of the framework and sparks may jump from one piece of metal to another in the interior of the ship. This is a serious danger, which can only be overcome by the most careful design of every piece of metal in the ship. It is not sufficient to ensure that every piece of metal is in good electrical contact with the rest of the framework, but all loops must be avoided and no piece of metal must be allowed to approach near to another piece without actually joining it. This is a preventable danger and is no doubt attended to by the designers.

(*c*) *Ignition of escaping gas through an external spark.*—If the electrical field is very strong the induced charges on the hull of the ship may be very large. In such circumstances brush discharges (St. Elmo's Fire) may take place from exposed metal projection, especially from sharp points. These in themselves are not dangerous as they are always pointed away from the surface into the surrounding air and can never occur within the outside surface of the ship. There is, however, the danger that they may ignite escaping gas and so lead to an explosion. The danger is not very great, but it as well to see that there are no ends of wire or sharp metal points on the outside of the hull, especially near the vents through which gas is released.

From what I have said I think you will have formed the opinion that the dangers to aircraft of the electrical forces associated with thunderstorms are not appallingly great; in fact, I myself consider that they are literally insignificant in comparison with the dangers due to the turbulent air motion, with its rapid ascending and descending currents, which occurs in the front and in the main cloud of a thunderstorm. Still disasters have occurred and all precautions must be taken; there is little that can be done when the aircraft is actually involved except to withdraw the wireless antennæ of aeroplanes and airships and the drag rope of a free balloon. The best advice of all is to keep clear of a thunderstorm altogether, for here if anywhere discretion is the better part of valour.

Discussion

Sir Gilbert Walker, in opening the discussion, said that he had two points on which he would like more information. The first dealt with the air movement in the upper part of a thunderstorm. The audience would remember the lecturer saying of the anvil shape at the upper part of the cloud that it was due to the spreading out of the formation where the ascending currents were checked on reaching a layer of warmer air. Anyone seeing for the first time the picture shown on the screen would imagine that all the air within the anvil was rising; but in that case the outer margin should be sharp. The orthodox view was that the outer margin was not formed by ascending currents of minute water particles, but by descending streaks of soft hail; for the air there was not quite saturated as regards water but was so for ice. Also occasionally you can actually see

clouds of cumulus form ascending through the anvil. He personally found it difficult to see how the margin of the anvil, if descending, could slope inwards, and would expect the hail to fall vertically, or nearly so; he saw nothing to cause the inward motion.

The second point was: Were thunderstorms in the tropics more dangerous to aircraft than in this country? So far as he understood, comparatively few people in the tropics were killed by lightning flashes; the conditions there would cause the thunderstorms to form at higher altitudes and therefore fewer flashes would reach the ground. We could, however, form some estimate from the size of authenticated hailstones. Hann said that the worst regions for hail were sub-tropical regions where the ground was raised above sea-level. There were properly authenticated measurements of hailstones two inches in diameter which fell in Yorkshire in a storm which took place in 1879. In 1897 there fell in Austria hailstones six inches long and four inches across, weighing over a pound. These measurements had been accepted in scientific journals. In India he had come across so many stories which could not be substantiated that he preferred not to make any definite statement as to the size of hailstones. As to the effects of tropical thunderstorms, he had come across cases in Gujarat where scores of men and hundreds of buffaloes were killed by hailstones. There was also the record of a storm in the United Provinces in which several hundreds of peasants were killed, not by lightning or shock, but by hailstones hitting them. Then there were the effects of wind in tropical storms; reports indicated that trees were blown down to an extent greater than in England. It seems fair to conclude, therefore, that aviators are liable to come across greater dangers from thunderstorms in the tropics than in these regions euphemistically called temperate.

Major G. H. Scott said he was interested in the lecturer's figure of 33 feet per second as the rate at which ascending currents capable of supporting hailstones half an inch in diameter rose. He thought such hailstones large for this country. He did not know such large ones were common. 33 feet per second worked out at about 2,000 feet a minute. In the specifications for the Government airships it was definitely laid down that the airships should be capable of rising and falling at 2,000 feet per minute. So that an airship should be able to keep its level even in that velocity, and also its height at an angle of 15°.

Another point was that in a warm rising current or a cold falling one the alteration helped to steady an airship very considerably. Where the velocity of the up and down currents was not very great and the static forces were greater than the dynamics and in a rising current the ship would tend to fall and in a downward current to rise. Of course, in a violent current in a thunderstorm the dynamic forces were enormous, but one had to remember that the larger the airship the larger the static force is to the dynamic. The larger the ship the less the effect. That probably explained why a balloon which was admittedly small was so badly affected by a thunderstorm.

He believed that the large airship of the future would stand very high velocities in vertical currents and it must not be assumed that even a violent thunderstorm would be able to affect it.

The pressure on the hull of an airship depended on the rate of acceleration and not on the alternate velocity.

He could not quite agree with Dr. Simpson's statement that no aircraft had entered a thunderstorm and come out safely. A number of German airships had been right through thunderstorms and come out intact. He was perfectly willing to admit that hailstones as big as hen eggs would constitute a serious menace, but hailstones of the ordinary variety did not bother them very much. The loss of visibility did not matter very much in the case of an airship, but it

had rather been his experience that the rising currents covered a smaller area and were much more violent than the downward current, and he would like to ask Dr. Simpson whether that was so.

With regard to the loss of the German Zeppelin L.1, he would like to point out that this was one of the early Zeppelins about equivalent to the first British rigid which flew, R.9, and it would have been unfit to contend with any violent disturbance. Also her ceiling was so low that she was obliged to fly dangerously near the sea. The modern ships were not at this disadvantage. He thanked Dr. Simpson for his paper, which he was sure would prove extremely useful to all operators of aircraft.

Lieutenant-Colonel V. C. RICHMOND thanked Dr. Simpson for giving them such graphic descriptions of thunderstorms, which had inspired those of them who needed it with quite sufficient awe. He was sure that Major Scott would agree that the best advice was to keep clear of thunderstorms if possible, and in this connection he would be glad to know if any information could be given as to the direction which storms took and whether there were any well-defined lines of either country or sea over which they seemed to follow a certain track at certain periods of the year, and also about the heights at which they might be expected to die out. He would like to know whether one could take the measurements of the theoretical case developed on page one as an average or maximum storm.

With regard to the effects of currents of electricity flowing through an airship, it was interesting to note that a definite case had been recorded of the girders of an airship being fused by the passage of electrical currents through the structure.

The total cross-section of the metal of the main longitudinal girder in the middle of R.33 was eight square inches, which is amazingly light from the constructional point of view, but he supposed it was pretty ample from the current density point of view. At the nose it fell to about a quarter of this.

He agreed that there should be no projections, and it was a matter of common experience among Zeppelin pilots, confirmed by his recent conversation with them, that airships had definitely been struck and have formed part of the vertical flash, and that flash apparently always struck the airship in the forward part and left by the control car in the front, or one of the wing cars.

Dr. Simpson referred to the question of internal fire which might arise from internal sparking, and had said that all loops must be avoided and no pieces of metal allowed to approach others without actually touching them. He thought he saw what was meant, but it might be difficult to carry out in practice. He said he thought it might be practical to insulate the bare hull of an airship, and pass high voltage alternating discharges through it in the dark, so that any points which show severe local sparking could be bonded together.

In a document by Bütler, one of the early Zeppelin pilots, of which he had a copy, he described very clearly his experiences in going through a thunderstorm, and the most noticeable feature was the St. Elmo fire which seemed to be most visible round the person of the machine-gunner and the gun which was on the top of the ship, and presumably no danger was found in firing the gun from that position and therefore he anticipated no danger from the St. Elmo fire.

Wing-Commander T. R. CAVE-BROWNE-CAVE asked Dr. Simpson to add some indication of the size of a thunderstorm. At what distance from the centre were there appreciable vertical currents and serious danger of lightning?

To avoid a heat thunderstorm was probably fairly easy, but the length of front on which a line squall advanced—some 600 miles in the example given—made it a much more serious obstacle. What was the usual speed of advance

of such a squall relative to the air, because that was an important factor as far as the aircraft was concerned?

Was the temperature of St. Elmo's fire sufficient to ignite hydrogen? Tests with gun-flash made early in the war showed that hydrogen-air mixtures of widely varying strength were not ignited presumably because the temperature was insufficient, although the flash was clearly visible in daylight.

Mr. FAHMY asked whether descending currents had ever actually been observed in thunderstorms. He wanted to know whether the sudden falling of balloons in these storms was due to loss of gas and instability rather than to descending currents.

Mr. GRIFFITH BREWER said that as the lecturer had asked for some personal experiences of balloons in thunderstorms he ventured to give his. Mr. Perrin, Secretary of the Royal Aero Club, and Major Claud Brabazon and Lieutenant Caulfield were with him. He was in charge of the Aero Club balloon when it used to go up once a week from the Crystal Palace. One Saturday they went off on a thundery-looking day, although they heard no thunder, and slowly drifted to the S.E. and were looking out for a place to land about an hour afterwards, when the balloon was suddenly lifted, the underpart began to shake, the flags at the side were raised. They started going up into the air at an alarming rate. Perrin held on to the neck line so as to keep the balloon from parachuting underneath and they went up in that very alarming state to about 8,000 feet, and then seemed to get outside it, and, having lost a great deal of gas, fell fast. They fell over some hop fields; fortunately they had enough ballast to escape, and landed, all rather bruised. They saw no rain, and heard no thunder, but it was a thundery type of cloud.

Lieutenant-Colonel H. T. TIZARD, before asking the lecturer to reply to the discussion, gave an account of an experience he had when flying with Major Hopkinson back from Orfordness one Sunday afternoon. They had been discussing at Orfordness the question whether one could get St. Elmo's fire on the wires of an aeroplane. It was quite nice weather at the start, but there were a lot of heavy clouds about. The machine was a B.E.2C., and the passenger sat in front with his back to the pilot. He turned to look round and saw a sudden flash of lightning, and knowing his passenger, said nothing but turned away from the storm; but Hopkinson also looked round and saw a second flash and motioned him violently to proceed towards the storm. He evidently thought such a chance was not to be missed. They first went round the edge of the cloud, and no fire appearing they then went inside the cloud. He remembered the air currents were "very complicated," but they were not very dangerous. They let the machine do as it liked. The rain was very heavy, so much so that they could not put their heads out of the machine. They were only in the cloud for what seemed to be a minute but was probably less. The airscrew and wings were slightly damaged. He remembered it was very dark in the cloud, which led him to ask the lecturer whether the darkness of a thundercloud was simply due to the amount of water it contained or whether the violent ascending air currents from the ground could carry up appreciable quantities of dust.

He also once took up Major Wimperis in a line squall which was rather interesting. In his flying experience he had noticed that series of parallel line squalls were quite common. On this occasion they went up between two squalls, caught one up and got above it; he could remember very vividly how those long rolls depicted in one of the slides stretched as far as one could see. There was very heavy rain and a violent wind, and they arrived over Martlesham when one of the squalls was passing over the aerodrome, so that they had to turn and wait for it to pass away. He remembered they were scarcely moving over the ground, the wind was so strong against them (? 70-80 m.p.h.), yet when they landed after the squall there was a dead calm near the ground. It was his

impression that one could generally get round a heat thunderstorm, and above a line squall.

Dr. SIMPSON in his reply said that Sir Gilbert Walker had referred to the movements in an anvil cloud. There were so few actual observations from such clouds that it was difficult to say what happened. What the meteorologists would like would be for aviators to fly near to such clouds and make observations, but he had not much hope of that being done.

About hailstones, he had not told them much about hailstones because he wanted his paper to be taken seriously. There was very good evidence for hailstones as big as a man's head, but in some cases at least measurements had been made some time after the storm had passed and it was possible that a mass of hailstones which had lodged in some hole and become consolidated had been measured. Frequently animals were killed in India, even buffaloes. These hailstones were sufficiently big to indicate what violent vertical hurricanes existed in the atmosphere.

He was interested in what Major Scott said and very much gratified that both he and Colonel Richmond were present. He had had considerable diffidence in writing on that subject when they knew so much more than he. He quite agreed that the real danger in up and down currents was not the velocity but the turbulence. The velocity was only significant in that turbulence in vertical, as in horizontal, currents was proportional to the velocity. There was great necessity for more experience.

Major Scott asked whether ascending currents were more violent than descending ones. They knew very little about the actual magnitude of the descending currents, but he thought it would be found that descending currents were very much smaller because the area was much larger.

Colonel Richmond and Colonel Cave wanted to know the size of thunderstorms and the length of the front of a line squall. Heat thunderclouds varied greatly from a couple of acres upwards, and in most cases one could fly round them, but one could not get round a line squall. It extended right across the country and they knew how big the one shown in the picture was. There was no chance of flying round it whatever. There were places where it was less developed than in others, but it would be difficult to choose those places. It would often be impossible also to fly over them. The mere fact of there being hail showed that they extended upwards to a great height.

Going back to Colonel Richmond's question regarding the length of life of a line squall, the one shown in Fig. 7 lasted fourteen hours; it was a very considerable and well-developed one. That was why they had worked it out in such detail. The upper limit of such a storm would probably be about twenty hours, but it could probably be detected as a weak local squall which did little more than ruffle the leaves of the trees for some hours longer. The condition of the atmosphere after a bad storm took a long time to become normal.

A metal cross-section of eight square inches ought certainly to be sufficient to take a flash without melting, but they did not really know how lightning flashes went through metal. It had all got to be worked out.

The question of loops and metal pieces. He was interested in what Colonel Richmond had told them. His idea of a loop was anything that nearly went back to where it started without joining.

With regard to Colonel Cave's question, he did not know of any experiments on exploding gas by St. Elmo's fire, which is, of course, of a very low temperature. They would be glad to hear that there was no danger to the gas in an airship from these brush discharges.

Mr. Fahmy asked if descending currents had been directly observed. No, so far as he knew, they had not been measured. They knew there were descending currents, but not how violent they were.

With regard to Mr. Brewer's experiences in a balloon, he thought he was not in the core of the storm as there was no precipitation.

The Chairman had asked about series of line squalls. In the diagram he had shown there had originally been two line squalls and he had taken out one to simplify matters. They often came in sequences but in decreasing intensity. A line squall always moved to the east, never to the west.

Addendum by the Lecturer

There were one or two points raised in the discussion to which I did not give adequate replies at the meeting, and I should like to add a few words about them.

Major Scott said that he did not agree that no aircraft had entered a thunderstorm and come out safely. I did not intend to give this impression when I referred to "the ascending currents within the cloud mass from which probably no aircraft has yet escaped after being once caught." I was then referring to the violent ascending currents which cause the separation of electricity and hold up the hailstones. These are extremely local and probably occupy only a very small fraction of the volume of the cloud, but that they do exist cannot be doubted. They must have a vertical velocity exceeding twenty-five feet per second and are probably extremely fluctuating and unstable. They would obviously put great strains on any aircraft, and I find it difficult to believe that an airship could stand the strain. The danger is not in the vertical velocity, but in the turbulence and local nature of these currents. Quite a number of aircraft have been within thunderclouds and even flown through them, but have any got into the real heart of the storm and come out safely?

This will probably answer Wing-Commander Cave-Browne-Cave's request for information as to how near to a thunderstorm aircraft can go before they get into danger from the air currents. The very dangerous currents are well within the cloud and I doubt whether the currents outside the cloud are ever really dangerous to modern aircraft. The whirl just under a line squall is certainly dangerous, but probably an aeroplane could negotiate it; I doubt whether an airship could.

It is impossible to give a definite answer to the question as to how far away from a thunderstorm the electrical tension becomes dangerous. Mr. C. T. R. Wilson is at present making experiments which will throw light on this subject. My experience has been that the field intensity falls off very rapidly as a thunderstorm passes away, and I doubt whether aircraft half a mile away from a thundercloud would be in any real danger from lightning.

A heat thunderstorm does move relative to the air, but only very slowly; the advance of a line squall is at the same rate as the velocity of the current on its western side.

Colonel Richmond asked whether thunderstorms follow any definite tracts. A great deal of work has been done on the question in Germany and it is clear that certain regions of country are relatively free from thunderstorms and others are often visited. I doubt, however, whether the results of this work are of much practical value.

Colonel Tizard said he thought they would agree with him that they had been listening to a most interesting lecture and wished to give their very warm thanks to Dr. Simpson.

REVIEWS

Air Power and War Rights

By J. M. Spaight. (Longmans. 25/-.)

There is a widespread belief that the labours of international lawyers are of no practical value and the conclusions arrived at of purely academic interest. Quite illogically this view received an impetus during the Great War owing to the opportunities which occurred for cheap sneers at the lack of what in legal parlance are called " sanctions " and the failure of some of the combatants to abide by the rules for the conduct of war which had been drawn up. But in actual fact the experiences of the war were wholly in the reverse direction and formed a striking justification of the labours of the jurists. This was shown by the constant appeals to the code and the frantic attempts always made by belligerents to justify any infraction of it. It must also not be forgotten that the persistent ignoring of the rules of civilised warfare by the Germans was a potent cause of the entry of America on the side of the Allies. The issues were confused by the hypocrisy shown by both sides when what seemed a favourable opportunity for the influencing of neutral opinion occurred. In no direction was this tendency more noticeable than in the case of aerial warfare, and the impassioned protests against the bombing of " open " towns. In this connection the German outcry—for example at the raiding of Freiburg, the site of the Aviatik factory—was comparable with the British protests, for the consumption of neutrals, against the bombing of London—which not only could hardly be described as an " open " city, but was also the seat of the Government, the headquarters of the military and naval forces, an important link in the line of communications, and the site of many munition factories, any one of which is now specifically recognised by the Hague Rules of 1923 as a legitimate object for aerial attack.

The appearance of this book from the pen of the International Law expert at the Air Ministry is therefore an event of no little importance and it should certainly be studied by everyone interested in the future of aeronautics. It seems a far cry now to the first Peace Conference held at the Hague in 1899, when the Contracting Powers agreed to prohibit " the discharge of projectiles and explosives from balloons or other new methods of a similar nature," but Sir Rennall Rodd, first British delegate to the International Commission for the Revision of the Rules of Warfare at the Hague in 1923, reminds his Government in his official dispatch that this clause was confirmed at the second Peace Conference in 1907 and remains in force at the present day so far as concerns war exclusively between Signatories to that Declaration. The countries concerned were Great Britain, the United States, Austria, Belgium, Bulgaria, Greece, Norway, Holland, Portugal, Switzerland and Turkey. The fact that the provision becomes null and void if either of the belligerents is joined by a non-contracting state explains its abrogation during the late war, and this, together with its obvious archaism, is presumably responsible for its being treated as a dead-letter by Mr. Spaight. It is only mentioned here as illustrating the first stage of the consideration of the new form of warfare. The second stage was when the analogy of the rules of land warfare was introduced and the defended or undefended condition of a town became the test of legitimacy of attack. This has now in its turn been superseded by the Hague Rules of 1923, which merely insist on aerial bombardment being directed at a military objective, wherever situated.

The value of Mr. Spaight's book lies in the clear exposition of the effects of the new Rules, combined with an historical analysis of the steps which have led up to them in each case, copiously illustrated by references to experience. In fact, so complete are the historical references that the volume almost constitutes a history of the more important episodes of the War in the Air—to adopt Sir Walter Raleigh's phrase—and is far removed indeed from the dry legal treatise of tradition. If one has a criticism it is that the author does not always show a sense of values in his choice of authorities and is inclined to give the same weight to the gossipy correspondent "at the back" as to the more serious historian. He also has a strange predilection for quoting foreign authorities for statements as to British policy and experience, and *vice versa*. Curiously enough, although he must have had, or at any rate should have had, ample opportunity for consulting official documents, in almost every case he quotes unofficial accounts from the daily press or from the works of private individuals when giving the facts of incidents necessary to illustrate his thesis. As these facts do not always coincide with those given in contemporary official documents—witness the statement as to the results of bombing attacks on the stranded "Goeben" on page 224—it is a pity that the latter were not preferred, since it is reasonable to suppose that at this distance of time permission to quote from them would not be refused.

In the twenty-two chapters which the book comprises the whole range of international law as it affects aerial warfare is amply covered, and where definite regulations have not yet been agreed upon Mr. Spaight gives authoritative indications of the decisions likely to be arrived at on the analogy of the rules of land and sea warfare. The book is an expansion of the same author's "Aircraft in War" published in 1914. Is it too much to hope that he will in the same way expand and bring up to date his "Aircraft in Peace and the Law" published in 1919, and deal with the law relating to public non-military and private aircraft?

The History of Aeronautics in Great Britain

From the Earliest Times to the Latter Half of the Nineteenth Century

By J. E. Hodgson. (Oxford University Press. £4 4s.)

A useful step towards a "world" history of aeronautics would be the production in each country of an accurate and carefully-documented account of development viewed from a purely domestic standpoint. G. Boffito in his *Il Volo in Italia* and E. Fuld in *Uit de Eerste Jaren der Luchtvaart in Nederland* have to some extent set an example so far as Italy and Holland are concerned, though in a less ambitious form than that with which Mr. J. E. Hodgson has now filled the gap for this country. To avoid disappointment, it should be stated that he has—very wisely in our opinion—set a limit to the scope of his work by stopping at a date prior to the actual achievement of heavier-than-air flight, leaving it to some other pen to complete the story up to and beyond that point. In fact, his detailed record may be said to end with the foundation and early history of the Aeronautical Society, which he rightly regards as marking a definite stage in our aeronautical history. It was at any rate at that point that development to a large extent ceased to be a matter of spasmodic individual effort, since the meetings of the Society provided a centre for the interchange of ideas. This aspect was well put by James Glaisher in his remarks at the meeting, preliminary to the formation of the Society, of January 12th, 1866, when he said: "The Council may expect . . . that all information concerning the Science of Aeronautics may be brought to a focus, and made available through the medium of the Society."

Before dealing with the contents, a word of praise must be given to the publishers of this beautiful example of book-production. In spite of the mass of

material which it contains, it is by no means unwieldy to handle. The type is admirably clear and well set out on the page, while it is impossible to speak too highly of the quality of the reproductions—from old prints, paintings, photographs, books and even a handkerchief—of which there are 150, thirteen being in colour.

Two useful introductory chapters are devoted to a survey of international history to bring the British events into perspective, the book proper beginning with the legendary flights of Bladud, Elmer and Damian—for whose name, by the way, the present writer has never been able to trace any authority and on which point Mr. Hodgson is unfortunately unenlightening, although Bishop Lesley writes of him merely as " a certain Italian," and, if memory serves aright, William Dunbar (the only other contemporary authority) as an unnamed Abbot of Tungland. Chapter I. concludes with an account of flying romances, most of which are tolerably well known, although Addison's essay in *The Guardian* will probably be new to many. It is curious that in the course of researches into Robert Hooke's work, referred to in Chapter II. on early scientific speculations, Mr. Hodgson has apparently failed to trace an experiment of his, which, according to Wing-Commander Hubbard (*Aeronautics*, IV., 186), is recorded in *The Philosophical Transactions* under date January 13, 1664, six years before the appearance of Lana's treatise. In his next chapter the author brings out the fact that the underlying experiments which ultimately led to the French invention of the balloon were due to a great English chemist, and the following four chapters give in detail a vast amount of interesting details of the various balloon ascents in this country between 1783 and the end of the century, much of which was only to be found scattered among practically unprocurable books and old magazines and newspapers. Not the least valuable feature of these important chapters are the biographical details of the individuals concerned, though it is a pity that the only reference to Lunardi's return to England in 1805 is relegated to a note in the bibliographical appendix.

It may be interesting to add to the information given of General John Money, hero of a famous balloon ascent from Norwich, that in 1787 he was (according to J. T. Smith, author of *A Book for a Rainy Day*) lodging in the same house (37, Gerrard Street, Soho) as Edmund Burke, the politician, who, Mr. Hodgson records, was among those present at Lunardi's first ascent. In the same connection we think he will find a third impression of the anonymous mezzotint reproduced in Figure 55 in the Norman Collection at the Patent Office. In referring to Dr. Jeffries he omits to mention that various relics of the first aerial channel crossing, including Jeffries's leopard skin " Montero " hat, were shown at the aeronautical exhibition held under the auspices of this Society at the Alexandra Palace in 1885. A more surprising lapse is the lack of mention of the well-known engraving depicting the burning of Keegan's balloon in 1785, which is the complement to the three caricatures recorded. We are sure Mr. Hodgson will be amused to learn that Samuel Rogers's anecdote of that curious character Dr. Fordyce, which he quotes, recently appeared in a London evening newspaper with a fresh attribution to a modern Harley Street consultant.

Chapter IX., dealing with the balloon in literature, caricature and fashion, provides a fascinating introduction to a side line of collecting and forms a prelude in lighter vein to the two following chapters on ballooning between 1800 and 1850 and the experiences of Charles Green, whose many ascents, which had become confused at the hands of other writers, are here reduced to order. Chapter XIII. deals with the numerous attempts which were made during the nineteenth century to render the balloon dirigible, in the course of which a well-deserved tribute is paid to R. B. Mansfield, though perhaps hardly enough stress is laid on what we believe to be the chief value of his *Aerial Navigation*—its extraordinarily complete record of the ideas of previous experimenters. The Aeronautical Society's first Council Minute Book reveals, by the way, the interesting fact that this book was

offered in manuscript to the Society in 1872 and was reported on most favourably by Wenham; but the Council reluctantly declined to undertake its publication owing to lack of funds. In saying that the only description of Delamarne's dirigible of 1865 is to be found in Burnaby's *Ride Across the Channel,* the author appears to have forgotten the handbill with a wood-cut of the airship which hangs in the Society's Library. The failure of Delamarne's balloon at the Society's exhibition at the Crystal Palace in 1868 aroused great indignation in Glaisher, who categorically accused the inventor of fraud and induced the Council to rescind a resolution of condolence which had been passed at a meeting from which he was absent. Chapter XIV. is devoted to a history of the parachute, which has been hitherto completely ignored in this country, and is a most welcome contribution to aeronautical literature, in which the exploits of John Hampton are accorded due recognition.

Perhaps the most interesting chapters to most readers of the Journal will be Chapter XII. on "The Decline of the Free Balloon and the Development of Aeronautical Science" and Chapter XV. on "The Evolution of Mechanical Flight," since they contain by far the best account which has yet appeared of the considerations which led to the formation of the Aeronautical Society on January 12th, 1866, and the work which was done by its early members. The importance of the investigations of Cayley, Henson, Stringfellow, Wenham and Moy are well assessed, and their value as forerunners of such men as Horatio Phillips, Laurence Hargrave, Hiram Maxim, Percy Pilcher and others clearly brought out. Mr. Hodgson's estimates will, we believe, receive general acceptation, though he appears to be wrong in attributing to Wenham the first anticipation of the cambered wing, which had certainly been previously visualised by Cayley—who also seems to have appreciated the suction effect on the upper surface referred to in Phillips's patent—while Henson's design of 1842 also embodied cambered wings. In view of this, we doubt if it is quite true to say that "it was not until 1884 that the idea of a cambered wing was definitely formulated by H. F. Phillips . . . as an invention for use in aerial machines," as Phillips was more concerned with the shape of the entering edge than with the bare question of cambered *versus* plane surfaces.

A curious doubt arises as to Figure 147 of Stringfellow's monoplane. This is reproduced as a representation of the successful model of 1848, but it certainly differs considerably in the shape of the wings from the illustrations of that machine given in F. J. Stringfellow's pamphlet and in Aeronautical Classic No. 5. On the other hand it certainly is not "the small model operated by a spring" of 1843, nor the Henson-Stringfellow king-posted model of 1843-1847. Equally certainly it is not the triplane of 1868; and yet these are the only four Stringfellow models of which there is any record. We are driven to the conclusion that it must represent the 1848 model at a different stage of evolution from that depicted in the other two publications referred to. It appears to be a less developed, and therefore earlier, form, but on the other hand behind it is seen a pair of wings which bears a striking resemblance to those on the model on the other photographs. Mr. Hodgson may find it worth while to inquire further on this point.

The book concludes with a useful chronology, an annotated bibliography which has long been badly needed, and a list (with brief summaries) of the papers published in the Aeronautical Society's Annual Reports of 1867-1893. These Reports contain a great deal of invaluable information which can be found nowhere else, and are insufficiently appreciated at the present day except by a very few.

This most readable and important work, which is certainly destined to become the standard history, is completed by a wholly admirable detailed index.

THE JOURNAL OF THE ROYAL AERONAUTICAL SOCIETY

A CLASSIFIED LIST OF RECENT ARTICLES

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Large All-Metal Seaplanes	A. Rohrbach	December, 1924

Engines

The High-Tension Magneto	A. P. Young	April-June, 1917
Aircraft and Motor-Car Engine Design Contrasted	Louis Coatalen	July-Sept., 1917
Possible Lines of Development in Aircraft Engines	H. R. Ricardo	March, 1921
The Instalment of an Aeroplane Engine	A. J. Rowledge	March, 1921
The Effect of Atmospheric Pressure and Temperature upon Performance	E. G. Ritchie	August, 1921
Engine Installation	R. K. Bagnall-Wild	April, 1922
Special Light-Weight Aero Engines	A. E. L. Chorlton	April, 1922
The Importance of Low Weight and Low Fuel Consumption	A. J. Rowledge	August, 1922
The Stability of Aero Engines	J. Morris	April, 1923
The Thermodynamics of Aircraft Engines	H. R. Ricardo	January, 1924
Radial Air-Cooled Engines	A. H. R. Fedden	April, 1924
Fuel Economy in Flight	H. T. Tizard	October, 1924

Gliders and Light Aeroplanes

Gliders and Light Planes	A. Ogilvie	November, 1923
Gliders: Their History and Uses	W. H. Sayers	May, 1924
The R.Ae.C. Light Aeroplane Competitions	J. S. Buchanan	December, 1924

Helicopters

The Problem of the Helicopter	L. Damblanc	January, 1921
Helicopters	John Case	Oct. & Nov., 1922
Air Travel, with Special Reference to Helicopters	F. M. Green	July, 1923

History

The Technical History of the Aeroplane	F. M. Green	January, 1918
The Langley Machine & the Hammondsport Trials	Griffith Brewer	December, 1921
The Work of S. P. Langley	L. Bairstow	November, 1922
Leonardo da Vinci as a Pioneer of Aviation	I. B. Hart	May, 1923
Leonardo da Vinci's MS. on the Flight of Birds	I. B. Hart	June, 1923
Sir George Cayley as a Pioneer of Aeronautics	J. E. Hodgson	August, 1923
The Development of High-Speed Aircraft	R. H. Mayo	March, 1924
Gliders: Their History and Uses	W. H. Sayers	May, 1924
French Aeronautical Societies	W. Lockwood Marsh	November, 1924

Instruments and Navigation

Air Navigation	H. E. Wimperis	August, 1919
The Static Head Turn Indicator for Aeroplanes	Sir Horace Darwin	December, 1919
Flying Over Clouds and Commercial Aeronautics	B. Melvill Jones	May, 1920
An Acoustic Tension Meter	A. H. Stuart	February, 1923
Aerial Navigation	A. P. Rowe	September, 1923
Some Recent Developments in Aircraft Instruments	H. E. Wimperis	Jan. & Feb., 1924

Materials

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1918	Steels Used in Aero Work	W. H. Hatfield	July-Sept., 1917
1918	Steel Tubes	W. W. and A. G. Hackett	April, 1918
1919	Timber: Its Identification & Mechanical Properties	W. H. Barling	May, 1918
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1920	Sockets for Wire Cables	Walter A. Scoble	November, 1921
1921	The Elastic Constants of Spruce as Influenced by Moisture	H. Carrington	December, 1922
1921	The Case for Metal Construction	J. D. North	January, 1923
1921	Fatigue in Metals	C. F. Jenkin	March, 1923
1922	Consolidated Wood	W. R. Turnbull	July, 1923
1922	Metal Aeroplane Construction	Hugo Junkers	September, 1923
1922	Some Materials in Aircraft Construction	J. D. North	April, 1924
1922	Materials in Aircraft Construction	L. Aitchison	April, 1924
1923	Dopes and Fabrics	J. E. Ramsbottom	May, 1924
1923	Large All-Metal Seaplanes	A. Rohrbach	December, 1924
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Medical

1923	Medical Aspects of Aviation	L. E. Stamm	January, 1919
1923	Some Physical and Psychical Effects of Altitudes	C. A. Swan	January, 1920
1924	The Human Machine in Relation to Flying	Martin Flack	December, 1920
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Meteorology

1917	Fog Conditions	G. I. Taylor	Jan.-March, 1917
1917	Meteorological Conditions which Affect Flying	C. J. P. Cave	July-Sept., 1917
1921	Meteorology in Relation to Aeronautics	W. H. Dines	January, 1918
1921	The Supply of Meteorological Information	H. G. Lyons	July, 1919
	The Meteorology of the English Channel	H. D. Grant	January, 1921
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1922	The Effect of Meteorological Conditions on Aircraft	G. H. Scott & V. C. Richmond	March, 1924
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Miscellaneous

	Looking Ahead	M. O'Gorman	Oct.-Dec., 1917
	Stress Optical Experiments	A. R. Low	November, 1918
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	Notes on the Storage of Aircraft	P. V. Hoare	June & July, 1922
	Radiological Inspection Work	V. E. Pullin	September, 1922
	Some Technical Aspects of Aviation in Canada	E. W. Stedman	September, 1922
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	Further Notes on Aviation in Canada	E. W. Stedman	September, 1924

Piloting

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	The Manœuvres of Getting-Off and Landing	R. M. Hill	October, 1921
	Methods of Aeroplane Flying Instruction	C. F. A. Portal	May, 1922
	The Manœuvres of Inverted Flight	R. M. Hill	December, 1923
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Research and Experiment

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1923	The Wind Channel: Its Design and Use	J. R. Pannell	July, 1918
1924	Full-Scale Aeroplane Experiments	W. S. Farren	February, 1919

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From Model to Full-Scale in Aeronautics	H. Levy	June, 1919
The Work of the Advisory Committee for Aeronautics	Sir R. T. Glazebrook	September, 1920
Scientific Methods in Aeronautics	G. I. Taylor	September, 1921
Standardised Stability Terms	Aeronautical Research Committee	November, 1921
The Importance of Research in Aeronautics	Council of the R.Ae.S.	February, 1922
Performance Testing of Aircraft	T. M. Barlow	May, 1922
Testing Aircraft to Destruction	W. D. Douglas	June, 1922
On Research and Other Matters	S. Heckstall Smith	August, 1922
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The Work of S. P. Langley	L. Bairstow	November, 1922
The Co-Relation of Model and Full-Scale Work	R. McK. Wood	December, 1922
Model Aircraft	R. A. Fraser	March, 1923
Wind Channel Experiments on a Pariah Kite	W. L. Le Page	March, 1923
Theoretical Relationships for Lift and Drag of Aerofoils	H. Glauert	October, 1923
The Vortex Pair Quickly Formed by Some Aerofoils	N. A. V. Piercy	October, 1923
Experimental Aspect of an Assumption of Prandtl's Theory	N. A. V. Piercy	October, 1923
Some Aspects of Aeronautical Progress	Sir F. H. Sykes	December, 1923
The Work of the A.R.C.'s Panel on Scale Effect	W. S. Farren	August, 1924
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Seaplanes

Naval Architecture in Aeronautics	J. C. Hunsaker	July, 1920
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Flying Boat Construction	David Nicholson	August, 1921
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Ten Years' Testing of Model Seaplanes	G. S. Baker	May, 1923
The Practical Aspect of Seaplanes	T. R. Cave-Browne-Cave	June, 1923
Design of Marine Aircraft in Relation to Seaworthiness	D. F. Lucking	November, 1923
The Launching of Aircraft from Ships	L. J. Wackett	November, 1923
Airmanship at Sea	R. B. Maycock	July, 1924
Large All-Metal Seaplanes	A. Rohrbach	December, 1924

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